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PREFACE

In 1908 Ebbinghaus introduced a brief textbook on general psychology with the terse statement, "Die Psychologie hat eine lange Vergangenheit, doch nur eine kurze Geschichte." Speaking internationally then, or even now for that matter, this cryptic sentence is a gross overstatement of fact. In more than three-fourths of the nations of the world Die Psychologie hat nur eine lange Vergangenheit.

The truth of the foregoing emerges when one scrutinizes the representation of different nations at the International Congress of Psychology at Stockholm (1950) or that at Montreal (1954). Less than one-sixth of the 69 Member Nations of UNESCO had representatives in attendance there and still fewer had active participants in the sessions. The provincial status of scientific psychology is also indicated by the scarcity of psychologists listed by *The World of Learning* in the membership rosters of the learned societies, research and technical institutes, colleges, universities, and national organizations concerned with popular education. In sovereign nations whose aggregate populations exceed 80 per cent of the world's population, psychology as a science has little, if any status whatever, among the older divisions of the humanities and sciences.

Can scientific psychology be introduced into nations where, as yet, not even a humble beginning exists? Certainly such an attempt is warranted in the light of the benefits some nations have derived from it where opportunities for its development have existed during the past five or six decades. And how should an international problem of this nature and magnitude be approached? Obviously, we shall need co-operative procedures beyond the simple expedient of supplying the learned societies, universities, technical institutes, and centers for popular education with complete coverage of current psychological journals, bibliographical services, and textbooks representing the best now available in four or five of the most widely read languages of Europe. A horse can be led to water, but the beast drinks ad libitum. First, where the desire for scientific psychology has not arisen spontaneously it must be aroused among statesmen, scholars, and college students in those countries where its implantation is contemplated. Otherwise it might not receive continuous spiritual and fiscal support.

Who should take the initiative in this matter? Those who choose and supervise exchange students may handle the problem of training the college students who ultimately should man the psychological centers in their homelands. Also, it is an appropriate objective of the International Union of Scientific Psychology. By way of making a start they could bring together at forthcoming International Congresses of Psychology representatives from as many of the 69 Member Nations of UNESCO as are willing to participate actively in the project. If no psychologist is available, then representatives

of closely related social or biological disciplines could be invited to serve as observers when not adequately oriented to qualify as participants. By repetition of this procedure for a few years and by effective utilization of exchange professors and students, one might confidently expect that leadership for the establishment and development of scientific psychology in every nation of the world will appear.

When scientific psychology flourishes on a world-wide basis, as in a few nations of the western hemisphere it has developed during the past half century, the *Annual Review of Psychology* will fully realize its major aims: to disseminate the fruits of research in psychology and to promote collaboration among scholars of all nations where pure psychology and its applications are vital to grass-roots organizations devoted to popular education and human welfare, and to programs of the learned societies, technical institutes, libraries, colleges, and universities.

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CHILD PSYCHOLOGY^{1,2}

BY MARIAN RADKE-YARROW

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Rather slowly, but very perceptibly, a new point of view is emerging in child psychology. It is not a point of view which is an irresponsible, radical departure from the conservative empiricism which has epitomized this discipline, but it is a reformulation of the problems in terms of a more dynamic conception of behavior and development.

Child psychology is an "old" field in the science of psychology. As such, it has some of the characteristics of maturity, as well as some of the features reminiscent of the problems and methods of psychology during the period in which the study of children emerged as a self-conscious discipline.

In brief historical perspective, one can identify four phases of development in this field. The first was the beginning of systematic accumulations of descriptive and normative data concerning children, as exemplified by the questionnaires, diaries and observations of the 1890's to the 1920's. The second came with the establishment of the various institutes for child research, supported by Laura Spelman Rockefeller Foundation funds in the era of the ascendancy of behaviorism. Data were carefully collected on every aspect of child development and behavior which was amenable to a behavioristic approach of objective measurement and testing. Sex and age differences were the primary points of comparison. Heredity as opposed to environment, and maturation as opposed to training constituted organizing frameworks for many research projects. Except for this theoretical controversy, child psychology was essentially atheoretical.

The third phase is not so easily located in time or by contributions. To call it a decline is too severe; it is rather a period when child psychology stood still. It continued in the same paths (of problems and methods), while in other branches of psychology psychodynamic theory of personality, social psychological field theory, learning theory, cultural anthropology, and techniques of social and field experimentation developed and expanded.

¹ The survey of the literature to which this review pertains was completed in May, 1954. The review covers the literature from the last dates included in the preceding Annual Review. Miss Gladys Morris assisted in collating the bibliography.

² The following abbreviations have been used in this review: CAT (Children's Apperception Test); HTP (House-Tree-Person Technique); TAT (Thematic Apperception Test); WISC (Wechsler Intelligence Scale for Children).

Childhood re-emerged as a crucial field of study (the fourth phase) when testable hypotheses based on clinical (mainly psychoanalytic) theories began to be formulated by systematically oriented researchers, when psychological theory and cultural anthropology converged, and when the genius of Kurt Lewin trained the experimental method upon meaningful social psychological questions within a framework of dynamic field theory.

It is with the perspective of its own history and in the context of developments in related fields of psychology and other sciences that we will take stock of the essential characteristics of current child psychology and attempt to evaluate the contributions, issues, and trends of the past year. The field to be reviewed is not automatically delimited by the subjects (children) of the research or by the disciplinary allegiances (child psychology) of the investigators. The orientation of this review is one of asking what contributions have been made to an understanding of developmental processes and of bio-social influences within a developmental framework. This choice was made over the alternative of examining the research in terms of its contribution to the body of data on the unique characteristics of children. The general characteristics of the work of this year are reviewed first without detailed critical analysis. Secondly, criteria of evaluation are presented, around which discussions and interpretations of the reported research are organized. Lastly, with a view again to the total field, the year's work is summarized and evaluated.

GENERAL CHARACTERISTICS OF CHILD PSYCHOLOGY

In assessing the state of child psychology, it has become almost traditional to lament the relatively meager time and effort given to research in this field as compared with other branches of psychology, and the relatively high proportion of naive, plodding, uninspired research efforts in this area. In one way or another, the preceding volumes of the *Annual Review of Psychology* express these points of view. In his evaluation of child psychology in 1950, Barker (8) emphasized a general lack of vitality in the field, pointing to the relatively low rate of publication and the feeble role of child psychology in the annual meetings of the American Psychological Association. We have reached somewhat different conclusions this year. Judging from sheer number of publications, a lack of vitality is not apparent. The study of child development has appealed to scientists from many branches of psychology and also from other disciplines. This scattering in professional interests no doubt accounts to some degree for the (still) small amount of activity identified as child psychology in the meetings of the A. P. A. Barker commented, also, that no new centers were being established and that only limited resources were available for research on child development. Here, too, the picture is radically different in 1954. Organized research programs devoted exclusively to child studies which have developed within the last five years, plus the continuing centers of child research, plus research funds from the foundations and the federal government, provide abundant re-

sources for research on children, far exceeding any previous period. For example, in the research-grants program of the National Institute of Mental Health, between 1948 and 1954, one-third of the research funds (approximately two and one-half million dollars) was directed to research relating to child development.

What is the direction of research in child psychology? A superficial, but none-the-less informative overview is obtained by ascertaining the proportion of research devoted to various topics or content areas. The United States Children's Bureau bulletin on research in progress (21) and the studies surveyed for this review were used as sources for the frequency tabulations (Table I).

Problems of personality dynamics and development are receiving by far the greatest share of attention (Entries a, b, c in Table). Studies of the rela-

TABLE I
INVENTORY OF CURRENT RESEARCH IN CHILD DEVELOPMENT

Problem Area	Per cent of Studies*	
	Children's Bureau Survey	Reviewed For Chapter
a. Personality dynamics (exclusive of parent-child relationships)	14	11
b. Personality deviations	14	16
c. Parent-child relationships	13	22
d. Intellectual functioning	12	19
e. Social behavior, leadership, group dynamics	10	8
f. Physical illness and handicaps	9	10
g. Attitudes, interests, values	9	6
h. Methodological studies	8	9
i. Peer relations and peer culture	5	8
j. Cultural influences on personality	5	15
k. Physical growth	5	6
l. Therapy or treatment	5	3
m. Motor development and skills	3	2
n. Cognitive processes	2	3
o. Communication processes	2	6
p. Perception	1	2
q. Hereditary and constitutional factors	1	2
r. Experimental modifications of attitudes, behavior	1	3
s. Learning	0.5	3

* A research project may concern several problem areas. Percentages are based on number of projects in which the problem area is investigated. In the Children's Bureau bulletin 314 projects were reviewed and in the journals covered for the chapter, 185.

tionships between child and parent constitute a focal point of present interest. There are many investigations of personality deviations, which include "delinquents," emotionally disturbed and psychotic children, and somatopsychic and psychosomatic problems of child personality. Few systematic investigations of the processes or outcomes of therapy are reported (Entry 1). (Clinical case studies have not been reviewed.) Studies concerned with influences of social class or cultural differences have received considerable attention (j).

The peer culture (i) has less frequently captured the interest of research workers (particularly theoretically oriented investigators) than has the child-parent constellation. Problems of social behavior and group dynamics (such as adult-leader influences, action research, and the process of incorporating adult norms) are relatively neglected topics (e, r).

Questions concerning intellectual development and functioning continue to attract research workers (d). The kinds of problems remain much the same. The areas of perception, learning, and motor development in children (m, p, s) are relatively neglected. Although only a few reports are concerned with the influence of constitutional and hereditary factors, there appears to be a change from an ultra-environmentalistic bias to a renewed interest in "constitutional" factors (q). These, in general, are the topics of current research.

The methods used in gathering the data are chiefly interviews, projective tests, questionnaires, and observations. In many studies combinations of techniques are used. Child psychology tends not to be an experimental science. When experimental designs are used they are often projective play situations.

Longitudinal studies, which are probably the most difficult to design and carry out, can be expected to make unique and fundamental contributions to child psychology. Seven major interdisciplinary longitudinal projects concerned with "total" normal development were found in reviewing ongoing research (see 21). Subjects are chiefly from white middle or upper-middle class groups. The norms and theories derived from these studies have, therefore, a narrow base for generalizing to other social and cultural groups. A great many reports which have come from these projects are on cross-sectional problems or on specific aspects of development. Several of these projects are now in major analysis phases. It will be necessary to await reports of these analyses to know whether our expectations of new data and insights from the longitudinal approach will be fulfilled.

RESEARCH CONTRIBUTIONS OF THE PAST YEAR

The extremely varied problems and data which comprise child psychology impose their own structure upon the organization of the review. For the time being, we will ignore the character of the total field and narrow our focus to a consideration of each of the separate areas. The research will be reviewed with the following questions in mind: (a) How are the problems formulated,

i.e., what kinds of objectives, frameworks, or motivations are discernible? (b) What are the significant findings? (c) What seems to be significant, new, or promising in contributions or trends? We shall attempt to interpret the findings (a) in the context of accumulated knowledge, both empirical and theoretical, and (b) with reference to the adequacy of the methodology employed.

RESEARCH ON PHYSICAL AND MOTOR DEVELOPMENT

From the standpoint of disciplinary lines, studies of physical growth are on the fringe of child psychology. They have been a central focus, however, in developmental normative research. The survey of this year's work leads to the impression that research in physical growth is not particularly active and that new questions are not being raised. Psychologically oriented questions, such as the interrelationships of physical and psychological growth, or the effects of growth changes and deviations on the child's self-image, are not considered.

The perennial search for a classification of individuals in terms of body types continues. Hammond (39) obtained measurements on 3000 British and American children. On the basis of factor analysis, body types similar to leptosomes, pyknics, and euryosomes in adults could be distinguished in children between the ages of 5 and 18. After intervals of three years, the constancy of body type is reported as high.

The interesting question of the degree of generality in adolescent physical growth is studied by Nicholson & Hanley (67) by means of a factorial analysis of the intercorrelations among various measures of maturation. The analysis points to the high degree of relationship among very different indices of maturation.

A theoretical issue of general biological significance is discussed by Collins (23). She hypothesizes a reciprocal relationship between somatic or cellular growth and integrative or functional growth. She points to the fact that during periods of rapid somatic growth, there is little development in integrative activity; and conversely as the rate of somatic growth decreases, there is an advance in integrative growth. On the basis of these data, the author suggests that the total energies available to the human body at any one time comprise a closed system, and that for those available energies somatic and integrative growth must compete.

The investigation of motor development and motor skills is conspicuously meager. One reason may be that the descriptive age-norm framework has been exhausted and that further research awaits a new orientation to the problems. Motor development in the contexts of personality functioning or of cultural determinants has not been probed systematically.

An approach from a cultural perspective is found in a study by Williams & Scott (98). Previous studies have noted gross motor acceleration of Negro infants as compared with white infants [Pasamanick (70)]. In the present study, the authors postulate "that early motor development, accelerated or

otherwise, is not a 'racial' characteristic, but, instead, is related to the manner in which an infant is handled and cared for." Data were obtained on Negro infants from two sharply contrasting socio-economic backgrounds. Differences in gross motor development were found to be related to methods of child care. The infants from permissive, nonrestrictive homes scored significantly higher than infants from homes in which punitive and restrictive child care practices prevailed.

LEARNING, PERCEPTION, AND COGNITIVE PROCESSES

Learning.—Learning theory has served as the framework and point of departure for a number of studies of social and personality development by Sears, Whiting, and others (discussed under personality). Little use has otherwise been made of opportunities for refining basic learning theory hypotheses by testing them in relatively controlled but somewhat more complex situations with children, using the "given" variable of developmental differences. Stevenson (83) has designed an experiment with children to study the problem of latent learning, a problem extensively studied in the rat laboratory. The design involved requiring children (ages 3 to 6) to learn a task: obtaining a reward from a locked box. A key had to be found in another box which contained several irrelevant objects. After several trials, the child was requested to find one of the irrelevant objects. The children's responses to the situation offer evidence of latent learning. There is a direct relationship with chronological age.

A study by Auble & Mech (5) tests the implications for the classroom situation of learning theories derived from laboratory situations. To study the effects of differential reinforcement, third grade pupils were randomly assigned to one of three groups immediately preceding a test situation. The reinforcement group was praised and encouraged. The omit-reinforcement group was simply present during this verbal reinforcement. The control group neither received reinforcement nor was present. The reinforcement group, although making significantly fewer errors, did not exceed the control group in correct responses. There were no significant differences between the differential reinforcement groups. Relatively little can be concluded from these findings for developmental theory. They do raise questions as to the validity of any simple generalizations regarding the effectiveness of various types and conditions of rewards.

Perception.—Although child psychology shows relatively little interest in perception, the investigations reported run the gamut from studies of classical perceptual problems, to studies of perception-motivation interrelationships, and to investigations concerned with developmental changes in perception.

The impetus for a study by Siegel & Ozkaptan (78) comes from an experiment with adults (74) in which it was found that subjects instructed to complete bisected geometrical forms did so with a minimal perimeter 87 per

cent of the time. This was interpreted in terms of "good Gestalt" and the principle of "closure." Siegel & Ozkaptan hypothesize that this finding can be explained in terms of past experience and conditioning to closed and symmetrical forms. Repeating the study with nursery school children, they found only 58 per cent of the forms completed with minimal perimeter. The finding is interpreted as support of their hypothesis. This study illustrates the use of the developmental variable in studying a basic hypothesis in perception.

The role of motivational factors in perception has been the subject of many studies in recent years. Here again developmental data from perception experiments are rare. Two studies on children confirm and, to some extent, elaborate on previous findings. In an experiment with 22 nursery school children, the effect of reward on size estimations has been explored by Lambert & Lambert (52). Estimates were obtained of size of tokens which were manipulanda in a token-reward sequence. The size tended to be overestimated after being associated with the reward sequence; experimental extinction tended to reduce significantly such overestimation. The estimation of the size of other tokens was influenced by their degree of proximity to the goal. A similar problem, that of the perception of the size of liked or disliked objects, was studied by Beams (10). The findings bear out the hypothesis that "affectivity" influences size perception.

Developmental changes in perception are investigated through children's responses to the Rorschach. A longitudinal study of responses of 50 children followed from 2 to 10 years of age is reported by Ames *et al.* (2). The authors conclude that "much of the child's perceptivity as measured by the Rorschach in the first 10 years appears to be influenced by strong, developmentally determined reaction tendencies rather than solely by the actual stimulus value of the cards themselves." The perceptions of the young child are primarily global. Responses of the youngest children tend to be vague and inaccurate, as judged by adult norms. There is a slight trend with age toward more attention to major details of the stimulus, along with increasing accuracy. The same problem is studied by Hemnendinger (44). Using cross-sectional data, he compares responses of children, ages 3 to 11, with those of adults. He describes the progression of development as a sequence from a "global, diffuse, undifferentiated stage through a differentiated and discrete stage to a level of hierarchically interrelated and articulated performance and functioning." The developmental changes described in both studies are consistent with theories of development enunciated by Werner (95) and Lewin (53).

Cognitive processes.—Investigations of the nature and development of children's thinking and concepts of the world continue very much along the lines initiated by Piaget. Some of Piaget's conclusions on animism are questioned by Kligensmith (49). He attempts to determine what the child means when he states that an object is alive; that is, does he assign to that object

certain sensory and functional attributes. The subjects (kindergarten to seventh grade) were asked: Is—alive; would it feel pain if I stick it with a pin; does it grow; etc? Results indicate that the younger children tend to use animal versus non-animal to differentiate animate and inanimate, whereas the older children (through the fifth grade) use the criterion of showing activity, e.g., a clock is alive. There was a progressive decrease with age in assignment of functional and sensory attributes to inanimate objects. The author concludes that when a child characterizes an inanimate object as alive he may mean much less by this term than do most adults, and much less than Piaget has implied.

Children's concepts of birth, bodily functions, and germs are reported in a series of studies by Nagy (66). Large numbers of children, aged 4 to 11, from Hungary, England, and the United States were interviewed with questions such as: What happens inside your head; what is the stomach made for; why do we breathe air in? They were also asked to draw the internal organs. The responses show little accurate knowledge of the shape or consistency of the internal organs or of the transformations which take place within the body. Very often the inside of the body is constituted in much the same way as the outer surface. As for children's concepts of germs, we learn little more than that "germ" is used by children for designating any micro-organism causing illness. Marked age trends do not appear in these responses.

To probe children's theories of birth, questions such as: how did your cat begin to live, and how did you begin to live were asked. The answers were classified in terms of four theories: "there is no birth as life has no beginning;" "a mammal's life begins without the mother"; "there is birth with the mother only"; and "mammalian birth which also implies the father." The first two theories are found only before the eight year level and the last theory only after eight years. The third theory is given most frequently of all.

One is somewhat at a loss as to how to interpret the findings in this area. Are some developmental principles demonstrated in the responses; are educational procedures reflected in the findings? There is need for more of a framework in which to interpret the descriptive data on children's concepts of a wide variety of phenomena.

INTELLECTUAL DEVELOPMENT AND FUNCTIONING

Under one topic are subsumed two very differently oriented research endeavors: the study of intelligence testing and the distribution of intelligence, and the study of children of various intellectual levels. Little that is new appears this year in the measurement of intelligence. The problems are primarily conceived of as problems in the refinement of a tool and in its use for diagnostic purposes. New tests of intelligence have not appeared. Investigations are directed instead toward subjecting existing tests to further study. The following reports are representative of many similar investigations. Using the Stanford-Binet as a criterion, the validity of the Wechsler Intelligence Scale for Children was investigated by Kureth, Muhr & Weisgerber

(51). Correlations between the two scales for five and six year old children ranged from .71 to .84, with the WISC² yielding somewhat lower scores than the Binet.

The Stanford-Binet and the Full-range Picture Vocabulary Test are compared for use with mental defectives [Sloan & Bensberg (79)]. Using 60 subjects, a correlation of $+ .76$, P.E. $\pm .03$, was obtained with somewhat higher scores on the Picture Test than on the Binet.

Dunsdon (27) explored the diagnostic value of differences in scatter in the 1937 Stanford-Binet, comparing large samples of normal, backward, and schizophrenic children. Scoring for response pattern failed to differentiate between the specific types of children studied.

A number of studies deal with psychometric problems in testing handicapped children. Graham & Shapiro, for example, (36) found that the performance scale of the WISC could be administered successfully through pantomime to deaf children.

Concerning the effects of bilingualism on the measured intelligence of children there is general agreement, both in a review of the literature (25) and in the individual investigations (1, 3, 4, 47): namely, that bilingual children tend to suffer from a language handicap when measured by verbal tests.

The comparative test performance of Negroes and whites is still a controversial area. While various findings indicate somewhat lower scores for Negro than for white children, questions which need to be considered are: When does the depression in scores occur and what accounts for it? On the first question, Knobloch & Pasamanick (50) offer data. In an earlier study the authors found no significant differences between Negro and white infants on the Gesell Developmental Schedule, except for the possible acceleration by Negro infants in gross motor behavior. In follow-up testing, a year and one-half to two years later, the Negro children had maintained a normal rate of development, compared with the norms on white children. The study of Williams & Scott (98) cited earlier on the effects of child rearing methods on Negro infants' motor performance is relevant here. A concrete basis for attributing test score differences to certain definable cultural differences is achieved in this study.

Research activity in the field of mental deficiency shows signs of significant trends. Interest has risen considerably. In a careful assessment of the various conceptions of mental deficiency, Perry (71) points out that although mental deficiency is regarded by professional opinion in general as a relatively incurable condition, this kind of conception is not congruent with present day thinking in other branches of mental health. The psychotic is no longer viewed from the standpoint of his symptoms alone outside the context of his total current situation and history, nor is the delinquent or alcoholic so regarded.

Among studies taking the less static view of the mentally retarded is a report by French, Levbarg & Michal-Smith (34), based on an intensive case study. It describes the marked improvement in performance and adjustment

of a mentally retarded boy during the year in which his mother was receiving therapy directed toward helping her evolve better methods of rearing her son. Exploration of the life situation of the retarded child is one direction which research appears to be taking.

Staver's (81) study of retarded children investigates the mother-child relationship. Mothers in many of the instances studied aided the helplessness of their children, in this way helping to maintain the infantile character of the child so that the mother could take care of him.

The effects of three different types of supervision, designated as strict, friendly, and laissez-faire, on the behavior of 36 high grade defective boys were studied by Tizard (89). Unfortunately, only rather limited ratings were made, such as talkativeness, work productivity, and quality of work. The experiment covered a three month period. There were no marked differences among the three supervision groups, except that "industriousness" was significantly less in the laissez-faire group.

The application of psychotherapy to the mentally retarded is of comparatively recent origin. As more therapists become interested in the problem, we will be better able to evaluate the possibility of the ameliorative effects of this approach on some kinds of mental deficiencies.

In rather marked distinction to this new flurry of interest in the mentally retarded, research on the gifted child shows no parallel development. Only one report (99), a correlational study in which gifted children were given a variety of special ability tests, was found. The data corroborate earlier findings showing trends towards excellence in abilities of various kinds among the gifted children.

It has been characteristic of child psychology to stress pathologies and problems more than the creative and constructive aspects of human behavior. Maximizing the potentialities of giftedness and providing the kind of setting in which creativity can flourish are socially important goals to which child development research could contribute much more than it has to date.

Communication processes.—Considerably more psychological orientation is apparent in this field than formerly. One might suppose that, similar to research on physical and motor development, normative and correlational studies of language development have been more or less exhausted and that it is now time for more integration of findings on language development with findings on personality and social development. Questions are formulated in this year's publications which demonstrate this interest.

McCarthy (59), who has contributed much to descriptive and normative research on children's language, discusses the hypothesis that sex differences in language development and disorders (which have appeared consistently) may be related to differences in the parent-child relationships of boys and girls. She suggests that greater warmth and affection and security may be given to girls than to boys. This hypothesis is especially interesting in the light of findings of pervasive differences in the parental treatment given to boys and girls after the first year, as reported by Sears and collaborators

(77). Here may be the hunting ground for various hypotheses concerning communication processes in the child.

The relationship of family environment and language development is reported elsewhere. Nisbet (68) demonstrates that the negative correlation between intelligence test scores and family size is attributable in part to the effect of family size on verbal development. He theorizes that limited contact between mother and child, which he assumes is the case in large families, has the effect of retarding verbal development. Further study is needed; however, this hypothesis is consistent with the findings of Brodbeck & Irwin (17), who have described the retarding effects of institutionalization on infant language development.

Work on speech and reading problems has for a long time recognized the components of "personality" in these part dysfunctions. To view "normal" language development in somewhat comparable perspective is a logical trend.

The normative problems studied concern infant speech. McCurry & Irwin (61) attempt to record what they call infant word approximations, i.e., phonetic patterns interpreted by observers as attempts by the infant to pronounce a standard word. Within the age range studied (19 to 22 months) there were significant increments both in the type and the frequency of attempted word approximations. No significant sex differences were found.

McCarthy (58) discusses the developmental sequences in vowel and consonant utterances of infants, relating them to laws of development. Vowel vocalizations develop cephalo-caudally, while consonants seem to develop proximo-distally, from the back to the front of the mouth.

PERSONALITY DEVELOPMENT AND PERSONALITY DYNAMICS

Personality studies have the center of the stage in current research on children. Contrary to some of the preceding problem areas reviewed, it is more the rule than the exception for personality investigations to proceed from a conceptual framework from which research questions are systematically derived and within which findings are interpreted. Psychoanalytic theory continues to stimulate the formulation of significant research problems (30, 101, 102), social learning orientation has provided scientific coherence for a variety of studies (77, 97), while derivations of Lewinian conceptualizations form the basis of others (9, 30).

There are several major foci in reported studies on personality. The organization of the review follows from the focal issues.

Early experience related to later development.—Experiences of early infancy and childhood are alleged to be crucial in personality formation. Psychiatric observations, experimental work with animals, and systematic data on socialization in human subjects all point to the importance of early experience. The so far available data do not, however, present unequivocal evidence of the exact nature of the processes by which early life conditions are related to later development.

This year, in addition to a number of studies concerned with specific

variables in early experiences, there are several impressive research endeavors directed toward developing the methods needed for adequate studies of the effects of infant and childhood experiences on subsequent behavior.

Experimental work with animals offers one of the richest sources of data. Experimenting with the social environments of infant animals (birds, sheep, guinea pigs, and puppies), Scott (76) and his colleagues demonstrate the alterations in later social relationships which can be brought about through experimental changes in early environment. For example, a lamb raised on the bottle and away from the flock, when returned was unable to "socialize," as evidenced by wandering off and not relating her movements to the flock. Subsequently, her behavior with her own lamb was described as indifferent, in contrast to that of the other sheep, paying little or no attention to her offspring.

Moreover, in the animals studied, Scott found the timing of experiences to be extremely important in the process of socialization. There are points in the developmental sequence when the organism is most receptive to certain kinds of stimuli or most vulnerable. The point at which a new relationship is formed is a critical one for the determination of good or poor adjustment in that relationship.

The implications of these animal studies have not fallen on deaf ears among child psychologists. Bowlby (15) discusses the relationship of the data on the critical phase hypothesis to psychoanalytic theory regarding effects of early experience. Stendler (82) discusses the implication of this hypothesis for questions of dependency in child socialization. She suggests that the specific effects on personality of disturbances in the gratification of dependency needs are related to their timing. She postulates that frustration of dependency needs during the first critical period (around eight months) affects "ego aspects of the personality" whereas similar frustrations during the second critical period (between two and three years) affect "superego aspects of the personality."

The findings of a study based on longitudinal data of the relation between nutritive sucking experiences in infancy and non-nutritive sucking in childhood are interpreted in terms of the phase specificity concept. Yarrow (102) found prolonged thumb sucking in childhood to be significantly associated with short feeding time (i.e., minutes per feeding) in infancy. These findings would tend to support Levy's deprivation theory of sucking. However, additional data suggesting a relationship between prolonged thumb sucking and late age of weaning support the reinforcement hypothesis offered by Sears. The author suggests that these theoretically contradictory data might be reconciled by the introduction of the phase specificity concept. If it is hypothesized that the strength of "oral drive" varies at different stages of development, inadequate gratification of this "drive" during the period of its greatest intensity may result in a fixation on this mode of gratification. On the other hand, continued gratification of this drive at a later stage of development may create conditions of overgratification and thus result in fixation.

Considering the crucial role attributed to infant experiences in psychological theory, and the wealth of descriptive data on infants, there has been comparatively little direct research on infants from the orientation of personality theory. It would appear that a start has been made in this direction, beginning with the arduous task of developing methods of studying infants.

Escalona and associates (29, 30) have reported on the methodology of an intensive study of normal infants. Their procedure consists of detailed observations of infants under normal environmental conditions, alone, and in interaction with the mothers, as well as psychological tests and interviews with the mothers. The objectives go beyond the mere compilation of minute behavioral data. In the authors' words,

It appears to us that the process of normal personality development cannot be adequately described, much less conceptualized in the form of ultimately testable hypotheses, unless we know the variety of behaviors that can be observed among healthy infants and the variety of environmental circumstances under which these behaviors can occur.

The observations have led to such provoking and, until now, neglected questions as, do mothers of active and robust infants choose child rearing methods characteristically different than those chosen by mothers of sensitive, fragile infants? How do infants who differ in various sensitivities experience the numerous contacts with the mother?

So far, only the detailed description of the methodology and impressionistic findings have been published; however, analysis of data is continuing. In its conceptualization, this study represents a happy combination of two theoretical approaches. The general formulation is derived from a Lewinian orientation, whereas the specific hypotheses to be tested ultimately are based on psychoanalytic theory.

Wolf's study of infants (101) begins with prepartum interviews and tests of the mother. Intensive observations are made during the hospital period and in frequently scheduled sessions thereafter. Short-term predictions of the infant's behavior and the mother-infant relationship are attempted, which can be checked subsequently. This is a promising attack upon the problem of early diagnosis.

This project, too, is only in its early stages in which the problems of methodology have been the primary focus. Wolf stresses the essential character of the personal relation of the observer to the observed. For the past several decades efforts have been concentrated on increasing objectivity of measurement and developing ingenious recording schemes. In these developments there has been some real danger of throwing out the baby with the technique. The need for recognition of the subjectivity and feeling in the act of observing is not to be confused with a return to haphazard impressionism.

To study the effects of specific aspects of infant experiences upon development, natural settings in which infant environments vary in measurable ways have been used to good advantage. Since the description by Spitz (80) in 1945 of the drastic and dramatic effects of separation or loss of the mother

upon the infant's responses, there have been a number of studies of the effects of institutionalization upon development. The conclusions are essentially similar: namely, that this kind of experience affects behavior during the separation and also during subsequent development. Bowlby & Cantab (16), from observations over a period of four years on 49 young children separated from their mothers under the age of four and a half, report this finding. They describe further that the child who, in addition to separation, lacks ordinary care, is especially likely to develop hostile and destructive behavior. In this way, a combination of separation with rejection is likely to occur.

Fischer (32) confirms the earlier findings that institution-reared children tend to show developmental retardation. During early infancy there were slight differences between her institution and noninstitution children. By three years the children who remained in the institution were significantly lower in developmental level, whereas those who were placed in foster or adoptive homes during infancy showed no permanent effects of the early institutional experience. The influence of selective placement is not controlled, however.

Data on the significance of early mother-child relationships come from a very different source: the large-scale social experiment in the collective communities in Israel (45) in which infants and children are reared with others of their own age. Only impressionistic data are reported: e.g., "Notwithstanding the indications of a certain degree of insecurity among infants and junior children, these indications seem to subside during the latency period, serious emotional disturbance does not appear to be prevalent. . . ." There is some suggestion that as adults these communal-reared individuals experience considerable difficulty in the realm of sexual and parental adjustment.

The findings and theories of the studies of early experience are highly provocative, but not definitive (85). Only a small segment of infant experience has been explored. Most of the observations are based on institutionalized children. Some obvious problems of methodology appear in these studies. We are left without knowledge, for example, of the stimulus conditions prevailing in the institutional setting. We need further studies to answer many remaining crucial questions.

Parent-child relationships and the socialization process.—The discovery of significant variables of parental and child behavior, and the formulation of hypotheses concerning the relationships between these two kinds of variables constitute the central problem of this research area. Until recently the approach has been primarily an empirical one (43), and parent-child variables have been loosely defined. Some of the current investigators, however, appear to be disinclined to continue along this path. They (56, 69, 77) have pointed out a number of the weaknesses in past assumptions and methodology, dealing chiefly with defining the parental variable. Serious inspection of these criticisms may help to bring this area of research out of the uninspiring

pattern of low-order correlations between isolated aspects of parent responses and child behavior, few of which are psychologically meaningfully interpreted. One error of which much past research is "guilty" is to assume or to measure parent behavior and attitudes as if they were fixed and unchanging. Not only does the mother vary in her responses toward different children, but she will vary markedly with respect to her handling of the same child. (For example, she may be highly punitive in regard to property destruction, but quite the opposite regarding sibling aggression [Nowlis (69)]. Secondly, her behavior and attitudes may vary markedly in accordance with the needs and developmental level of the child. (The mother who has found the dependent infant a "difficult" organism may respond quite differently to the older child who has developed considerable independence.) The implications for retrospective appraisals by the parent of her attitudes toward the child are apparent. Thirdly, the child cannot be dealt with as an abstraction. The interaction of the child's individuality and the parent's needs must be taken into account. Coleman, Kris & Provence (22) state the issue clearly:

The child that readily accepts the cuddling of the mother will elicit different responses in her than the child that refuses such intimacy. . . . To illustrate opposites we refer only to two approaches in current child psychiatry; the one considers severe childhood disturbances largely, if not solely, as a product of the behavior of certain mothers and here the term "schizophrenogenic mother" has arisen; the other views the mother's behavior also as response to a child that does not or cannot adequately respond to her. Obviously both factors interact and their interaction has to be studied.

Surely one would view with a jaundiced research eye future investigations which do not attempt to take some of these and other refinements into account.

Sears and collaborators (77) report a study of child-rearing antecedents of child behavior. Maternal practices referring to various kinds of nurturance, frustration, and punishment were examined in relation to dependency and aggression in early childhood. Three main conclusions are stated: that the kind and amount of frustration and punishment experienced by the child are major determinants of the properties of dependency and aggression drives; that there are radical sex differences in the processes by which these drives are developed; and that there are pervasive differences in maternal treatment of boys and girls after the first year of life. This research is distinguished by the clarity of its theoretical formulation. Learning theory was used as the framework for selecting, defining, and measuring the variables, and for evaluating their results. In terms of their theoretical framework, the authors are in a position to plan next steps in research in a way which builds upon the present study.

Lorr & Jenkins (57) approach the question of determining significant dimensions of parent-child relationships by a factor analysis of the 30 Fels home rating scales. The following dimensions emerged: encouragement or denial of dependency needs, democratic or authoritarian values, and strictness or laxity of the home.

Empirical studies have much to contribute concerning the little studied question of the father's role in socialization. Tasch (86), on the basis of interviews with urban fathers, concludes that fathers play a more active role than is usually granted in our stereotypes of the modern American father, and the majority consider child rearing as a part of their role. Sex typing activities in which they engaged with the children were apparently not consciously understood as such. Unfortunately, social class characteristics of the sample are not clearly described. Study of class differences in the father role and in sex typing activities would be a significant contribution to socialization theory.

Some of the consequences of father-separation for the child's personality development are studied in a "natural" experimental situation in which fathers were away from their families in service in the armed forces during at least the first year of their first born's life [Stolz *et al.* (84)]. Data were obtained through interviews with parents and by observations of preschool children of the father-separated and a control group. The fathers were college students and staff members. Upon the father's return, the child's behavior toward him was often withdrawing and unresponsive. These fathers tended to be critical of the child's dependence on the mother and assumed primarily the role of disciplinarian. In group situations, the war separated children were unable to establish or maintain relationships with other children as skillfully as the nonseparated children. With adults, the war separated children were relatively more dependent and demanding; when confronted with authority, more compulsively obedient and openly defiant.

Culture-child interactions and the socialization process.—In addition to studies of the influence on personality development of idiosyncratic characteristics of the parents, there have been studies of the influence of variations in parental behavior derived from differing modal norms and values of social and cultural groups.

The most extensive work in this area is an analysis by Whiting & Child (97), of child rearing practices of 75 primitive societies, based on data from the Cross-Cultural Files of the Yale Institute of Human Relations. The practices relating to oral, anal, sexual, aggression, and dependency controls in the different societies are compared in respect to initial satisfaction potential and degree of socialization anxiety. The hypotheses tested, derived from psychoanalytic theory, are reformulated in terms of social learning theory. The major hypothesis relates to the early conditions of fixation, distinguishing between negative and positive fixation. The evidence suggests permanent effects on the personality of practices in the oral, dependency, and aggression spheres. With respect to the origins of guilt, the findings are less clear-cut, but offer some support of the psychoanalytic interpretation of guilt in relation to the identification process. An interesting perspective on American white, middle class child rearing was obtained by comparing the primitive cultures with the American group studied by Davis & Havighurst (26). The

middle class American falls in the upper extreme in respect to severity on most socialization variables considered.

Maccoby & Gibbs (60) report findings which suggest a shift in child rearing practices in the past decade in the upper middle class, white urban group. They conclude that this group, compared with the upper-lower class families, shows more warmth and affection, is less severe in toilet training, and in sex and aggression controls, imposes fewer restrictions on noise and neatness, and is less punitive. These conclusions are in direct contradiction to the earlier findings of Davis & Havighurst that the middle class is more rigorous in training for feeding and cleanliness than the lower class.

Considering socialization in a framework other than that of infant feeding and toilet training procedures, Farber (31) compares British and American values. Based on data from a sentence completion test (e.g., "A properly brought up child should be—") with insurance clerks in England and the United States, he concludes that in the socialization of the British child the emphasis is upon control of antisocial impulses, whereas for the American child the pattern aims at "a smoothly functioning individual equipped for getting ahead with a varied armament of social skills." The American stress is upon adjustment to other children; the British emphasize respect for parents and authority figures.

A social class framework is used to study personality and value patterns of children. Klausner (48) found class differences in self-concepts of adolescent boys, with the upper socioeconomic group showing a tendency for more self-aggression and more "psycho-social isolation" than the lower class. In another study [Schneider & Lysgaard (75)], middle class boys, in response to a questionnaire, showed greater acceptance of the ideology of delaying gratification than did lower class boys. Mussen (65) compares TAT² responses of Negro and white boys (ages 9 to 14) from the lower social class. The white boys' perceptions of the world are predominantly friendly, as contrasted with perceptions of a hostile threatening world by the Negro boys. Drawings on the HTP² test by white and Negro children (grades one to eight) show significantly greater aggression in the Negro than in the white group [Hammer (38)]. The findings are interpreted as support of the frustration-aggression hypothesis extended to the social-racial area.

Personality tests.—Paralleling the research in personality development, there is an active interest in the development of personality tests. The factor analysis approach used by Cattell at the adult level for defining dimensions of personality structure is extended to the childhood age range [Cattell & Gruen (20)]. Factor analysis of behavior ratings by peers (9 to 14 years of age) yielded factors essentially similar to those found for adults. Cattell & Beloff (19) report a personality questionnaire similar to the adult 16 factor questionnaires. The test awaits validation by external criteria.

Bellak's rationale for using animal pictures in the CAT² (that children will respond better to animal than to human pictures) is questioned by

Biersdorf & Marcuse (12). Comparing the responses of 30 first grade children to six pictures from the CAT and six comparable pictures using human figures, no significant differences were found in productivity. No comparisons were made of the "depth" of the responses to two sets of pictures. This latter question was studied by Light (54) by a comparison of written stories of 75 fourth and fifth grade children to CAT and TAT pictures. Here the stories were analyzed with regard to feelings, conflicts, themes, etc. The findings also do not support Bellak's hypothesis.

PSYCHOLOGICAL PROBLEMS AND DISORDERS

The studies within this classification are similar only in that they represent investigations of deviant behavior or conditions. They are, in many respects, psychologically different problems.

Personality disturbances.—Much of the literature on emotionally disturbed children is in clinical writings concerned with diagnosis, etiology, and therapy. These reports, although contributing the basic data from which formulations of the more systematically-oriented research derive, are not within this review.

In two studies the early parent-child relationships of boys who later became schizophrenic are explored. Frazee (33), using clinic data, compared 23 male children who later became schizophrenic with a control group. Her findings of a high frequency of overprotective, infantilizing mothers in the schizophrenic group is consistent with those of other investigators. The theoretical expectation of passive, ineffectual fathers is not borne out. A high frequency of severely cruel and rejecting fathers is reported. In the second, a retrospective study of parent-child relationships, Mark (63) describes mothers of schizophrenics, as compared with a control group, as more restrictive and exhibiting both excessive devotion and cool detachment. These findings are based on questionnaires from mothers after the sons were hospitalized for schizophrenia. The findings are particularly subject to the criticism that a differential effect of the illness itself may be operating in the recall report.

A physiological approach to behavior disorders is taken in a study by Ellingson (28). Beginning with the theory that atypical physiological reactions, especially under stressful conditions, may predispose to the development of abnormal behavior patterns, four groups of children, a normal group, a behavior problem group, normal children with one schizophrenic parent, normal children with one epileptic parent, were subjected to physiological stress conditions (hyperventilation and the cold pressor test). Basal measures of the groups did not differ significantly, but under stress differences in somatic and autonomic responses were found. Maladapted physiological responses seem to be greater among behavior problem than among normal children. Children of schizophrenics tended to react more like the behavior problem children.

Physical illness and handicap.—Among the potentially psychologically traumatic events of childhood we may count physical illness and disability. Several critical surveys of this field have appeared. Barker *et al.* (9) formulate the psychological problems of physical handicaps and illness in terms of Lewinian field theory. Tenny (88) formulates the psychological problem of the handicapped child as one closely akin to "minority status."

The emotional significance of acute illness and surgery are often commented upon in pediatric and psychoanalytic case materials. These experiences are interpreted as representing for the child punishment for misdeeds, attack upon his person, etc. (94). Prugh *et al.* (72) carried out a study of two matched groups of 50 children each, two to twelve years, hospitalized for relatively acute illness. They were observed under two different conditions of ward management, a "traditional" program and an experimental program. The latter included such changes as daily visiting from parents and psychological support for diagnostic and therapeutic procedures. Children's reactions on the ward and after discharge indicated marked differences in favor of the experimental group. There was not always a close correlation between ward adjustment and posthospital reactions, e.g., some children who appeared to be adjusting in the hospital exhibited severe disturbances later. In general, children with the most satisfying relationship with their parents showed the most successful adaptations.

Other studies of prolonged hospitalization (or institutionalization) formulated in terms of their potentially harmful effects have been discussed under personality development.

Juvenile delinquency.—On the etiology of delinquency and the social psychological correlates of delinquency there continue to be broadly formulated studies (73, 92, 93). Concentration on understanding social background factors and personality characteristics of the "delinquent" and on the development of techniques for predicting delinquency have developed out of a purely phenotypical approach. Recognition of the fact that the "delinquent" is not a simple psychological entity has not influenced much the formulation of research problems.

The predictive value of the Minnesota Multiphasic Personality Inventory has been explored in a study of 4000 ninth grade children. Two scales seemed to have predictive value, the psychopathic deviate and hypomania (41). In another investigation, the Blacky Test was found to differentiate between a sexually deviant group and a control group (55).

DEVELOPMENTAL SOCIAL PSYCHOLOGY

One looks in vain for evidences of a vigorous developmental social psychology to parallel the advances in the field of social psychology in general. This is the case in spite of the fact that the necessary research tools and underlying theories are available, and in spite of the fact that considerable social significance attaches to many questions of social development.

Peer-group influences and relations.—Studies concerning determinants of sociometric patterns within children's groups abound. They offer a variety of isolated data on sex choices [Bonney (14)], the influences of the adult community pattern on classroom choices [Dahlke (24)], the correspondence between peer sociometric and teacher-child choices [Gronlund (37)], the influence of religious group loyalties upon academic role assignments given to peers [Toby (90)], and the stability of children's sociometric groupings over a period of time [McGuire, Lanmon & White (62)]. The high degree of ability of children (grades 3 to 12) to perceive their own and others' sociometric positions within classroom groups is reported by Ausubel & Schiff (6).

An approach to problems of peer group culture and influences in terms of developmental questions such as the development of peer group norms, the development of leadership, and the influences of peers on personality, is a research area mainly for further development.

Social attitudes.—A steady interest in the study of children's ethnic attitudes continues. Some of the studies are descriptively oriented; some are concerned with the theoretical problem of personality dynamics and prejudice or with the conditions of learning of attitudes; and some are formulated in terms of group dynamics.

Zeligs (103) compares responses of sixth graders tested in 1931 with a comparable group tested in 1944. On an intergroup attitude test concerning 10 different racial or national groups very little change in attitudes or stereotypes was found over the 13 year period.

Interviews on attitudes toward minority groups and scores on the California Ethnocentrism Scale were obtained on white gentile children, 10- to 15-years of age (35). Ratings of prejudice based on the two sources correlated $+.67$. Intercorrelations on ratings of prejudice toward various minority groups ranged from $.69$ to $.97$. The authors point out that children's attitudes are usually mixtures of prejudice and tolerant ideologies, and that how this struggle is resolved in the child does not depend on psychological factors in isolation.

The relation between children's attitudes and parents' attitudes and sanctions is studied by Bird, Monachesi & Burdick (13). Their focal point was the effect on the child of discouragement by white parents of play activities with Negro children. Correlations between questionnaire scores of parents and children were low but positive.

Leadership influences on group behavior.—Studies of the influences of adult leadership on children's opinions and attitudes are carried out in a variety of contexts, mainly with the orientation of previous studies in group dynamics on the effects on group behavior of leader techniques and group atmospheres.

Repeating a study which had been done with college students, Hare (40) compared the effectiveness of supervisory and participating leadership in changing the opinions of 13-year-olds in Scout troops. Comparing question-

naire responses before and after a discussion concerning camping equipment, participatory leadership is reported as the more effective in changing opinions.

The tenth grades in four schools differing in ethnic and cultural characteristics were studied by Hayes & Conklin (42). One class in each school was designated as an experimental group, a second as a control; both were taught by the same teacher. In the experimental group a three week unit in biology, social science, or English was planned with the objectives of increasing social sensitivity and intergroup understanding. On two attitude tests and one sociometric test, administered before and after the experiment, the experimental groups showed greater change in the direction of favorable attitudes than the control groups.

A second part of the project compares three techniques of influencing attitudes: direct experience, an intellectual approach, and vicarious experience. Changes measured by sociometric choices among classmates showed the most effective of the techniques to be vicarious experience. The authors comment on the difficulty in "managing" realistic direct experience within the limits of their experimental program, hence, suggesting caution in generalizations of their findings.

In an experiment by Trager & Yarrow (91), substantial changes in attitudes and behavior were found in first and second grade children following a 14 session experimental program. Two contrasting experimental procedures were used, one in which the adult leader accepted status differences and social distance among groups, and the second in which the adult leader created an atmosphere of intergroup understanding. Comparable groups of control children had group sessions without "intercultural" content. Changes measured by projective test situations before and after the sessions and by systematic observations during the sessions indicate changes in children's responses in the direction of the atmospheres established by the leader.

DIDACTIC MATERIAL

Much literature on child psychology is published each year in the form of guides to parents and professional workers. These include a wide range in quality; there are some very excellent "translations" of scientific data and principles into nontechnical formulations, which do not compromise on accuracy, such as those of Teicher (87), Jenkins, Shacter & Bauer (46), Wolf (100), and Bakwin & Bakwin (7). Judged from an academic armchair, communication of the findings of psychological research to the "consumer" audience is at a high level of competency, though not uniformly. What are the results of these communications? Virtually nothing is reported pertaining to an evaluation of the effects of the feed-back of research findings on child-rearing, for example, on the child-rearing practices within the culture.

Midway between a nontechnical survey and a text book is the report of the White House Conference on Children and Youth (96). Consistent with

current research trends, the focus of the Conference was on personality. The research experience and perspectives of a group of biological and social scientists were contributed to this interpretive survey. The potential and special contribution of the multidisciplinary effort could have been to present as integrated an understanding of personality functioning as the combined advances of the biological and social sciences make possible to date. Unfortunately, this opportunity, admittedly a difficult one, was not fully utilized. The separate chapters remain discrete discussions unsynthesized by a systematic view of personality. Personality, unfortunately, is conceived of as an entity upon which various factors "act" (income level, constitutional factors, prejudice, etc.). Inasmuch as this book is intended as an interpretation of research, it is regrettable and, indeed, surprising that the authors of the chapters or individual parts of chapters are not identified. A question with which a thoughtful reader will be concerned is: in terms of whose perspective and whose field of experience are the generalizations and interpretations of each chapter made?

Text books, generally, in child psychology have shown little interest in theory. The organization of the data has, with few exceptions, been a topical rather than a systematic one. Recent texts, however, have begun to depart somewhat from the traditional treatment. Martin & Stendler (64), for example, have abandoned the usual organization of material, treating development as a social process. Around this organizing principle they have interwoven the materials of psychology, the biological sciences, sociology, and anthropology.

The revised *Manual of Child Psychology* (18) again fulfills a most valuable function of digesting and bringing up-to-date the extensive data on each of the various aspects of development and providing an excellent over view of the methodologies used in the study of children. A book of this kind must of necessity present child development in a highly compartmentalized fashion, although, in line with the research contributions of the past decade, one might have hoped for a chapter in which research on the interdependency of various growth processes had been presented.

There is certainly a more serious omission in the *Manual*, however. The child, as its chapters describe him, is without personality and almost without parents. There is no chapter on personality development. Out of the 1215 pages of the text, less than 25 pages, scattered through the book, are given to parent-child relationships. This de-emphasis is far out of line with current research trends.

On the side of commission, it would appear, too, that the *Manual* has taken a highly partisan, rather than a scientifically objective, point of view concerning the contributions of psychoanalytic theory to the research and knowledge in child psychology. Thus, psychoanalytic thinking is disposed of at one point as follows:

The literary man and the layman err in anthropomorphisms which recognize only a difference in size between the newborn infant and the adult. Their conception of the nature of the neonate is only a popular counterpart of the discarded speculations concerning the homunculus.

The psychoanalytic interpretation of the child is essentially an elaboration of the ideas that have just been described. (p. 273)

It need hardly be pointed out that an objective appraisal of the field will show many direct and indirect contributions of psychoanalytic theory to child psychology.

TRENDS AND PERSPECTIVES IN CHILD PSYCHOLOGY

It is hazardous to talk of trends from the publications of one year. It is presumptuous to judge what is a significant contribution at the time it is made. To attempt to achieve some perspective on current contributions we have considered them in terms of several of the long standing and central problems of child psychology.

One of these questions is concerned mainly with continuity in change, with the child as "father to the man." Studies in prediction from infancy and early childhood to later development are part of the question. This problem has attracted and concentrated many diverse skills and viewpoints in the past year (animal psychologists, anthropologists, child psychologists). Biological and psychological theory has given direction to this research. Great discoveries have not been made, but basic work is being done from which, there is reason to believe, "discoveries" will come.

This question includes, also, how changes come about, the nature or process of "transactions" between the organism and his environment. We have noted efforts in this year's reports to go beyond descriptive statements of change, to attempt to use or derive explanatory principles of how change and learning occur. It is, however, easier to point out the unknowns and the neglected problems than to identify specific advances. Thus, socialization in the broader sense of acquiring the values and symbols of society receives, perhaps, less attention than is due. The problems presented in this field are so varied, numerous, and difficult that the present state of scattered and fragmentary contributions (and not exclusively so) is not cause for great alarm. It is perhaps a necessary preliminary stage.

Child psychology revolves around another general question: one of looking for the essential likenesses and regularity in children's "equipment" and functioning at successive stages in development. Normative studies have not played an important part in current reports. Basic psychological problems of perception and learning to some small degree are being carried out within a developmental framework.

Understanding the deviations and individuality in the products of development is a third main theme in child psychology. It would appear that in investigating specific gross deviations (delinquency, physical handicap,

emotional disturbance, mental deficiency) there is somewhat less "fractioning" of the child, and more concern for the life situational factors in which these deviations exist or develop. There is, however, a deficiency in rigorous research design.

A large "residual" category for research remains which is more thoroughly defined by needs than by contributions. It concerns the idiosyncratically selective and patterned responses of the individual. Questions of how the child develops his individual modes of responding and of solving his problems, of how he selects his objectives and pursues his goals, how he develops his defenses, and how he evolves his private symbolic world are challenges for future research. Thus far there are only small beginnings in carving out of this vague and fussy area of individuality—meaningful research questions. Potentially this area may be a source of most significant contributions in child psychology.

In general, there are no outstanding changes in methods employed in this year's studies. There is evidence of a trend toward controlled and ordered subjectivity in observational approaches as opposed to dehumanized observational categories and points of view. There is some expression of impatience with life-time longitudinal designs (11), and a tendency for short term longitudinal studies to be designed instead, covering only two adjacent periods of development (such as the prenatal period through preschool). This may or may not prove to be a wise economy.

The published investigations are not without methodological inadequacies. The findings of many studies are invalidated by methodological errors. The weakest studies technically have not been included in the review. One methodological (as opposed to sheer technical) weakness in the field would seem to be the still considerable unhappy use of oversimplified approaches to complex problems. There are many publications in which the research imagination does not go beyond the particular subjects, conditions, and responses in the particular circumstance at hand.

The concentration of research upon a limited segment of the population (white upper middle or middle class) appears to be decreasing. Concurrently there is somewhat less tendency to proceed as if child behavior existed in social space in the abstract. Social and cultural perspectives are more often included as part of the formulation of problems and analyses.

The preponderant use of interview and questionnaire approaches, commented on earlier, tends to obscure the applications of experimental laboratory designs to problems in learning and perception, to problems in group processes and leader techniques, and to the study of children's behavior problems.

In the study of personality, particularly, natural settings (institutional settings, family variations, classroom conditions), providing the significant variables and permitting approximately controlled conditions, have been used very fruitfully.

The signs of greater convergence between data on child behavior and psychological theory have been noted in the small but clear beginnings of new problem formulations in such areas as children's motor and language development, intellectual functioning, and social and personality development.

Problems for research in child psychology are not scarce, nor are interests and skills wholly wanting. Advances are made slowly with many false starts. It might be well for the discipline to make more and better use of the valuable empirical data which have accumulated in this field, by looking at these data in terms of new conceptions and theories which have developed since the data were collected. In addition, so many children who have been so much and so intensively studied in the past have now grown up. Is there not here a source of data on many important developmental problems? As the earnest letter of a curious "constituent" inquired of a federal agency, "Whatever has happened to Johnny and Jimmy?"

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LEARNING^{1,2}

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INTRODUCTION

The range and scope of this literature, even within one year, is tremendous; the learning process has been observed from earthworms (101) to college sophomores. The earthworms learned about anxiety and so did some of the sophomores (28, 97, 123). White rats learned to swim under water and hold their breath (74), high school students learned, more or less, to stack a deck of cards (50), apes learned discrimination problems that nearly defy verbal description in their complexity (90). In spite of all this, psychologists have been chided by anthropologists for not explaining why people believe in ghosts (126).

Unfortunately for the purposes of a brief review, this literature is also tremendous in quantity; over 200 articles have been read in the preparation of the chapter. Obviously, selection for inclusion had to be most severe and in spots probably capricious; too often we have been able to report only one of several well-conceived and well-executed studies on a problem. This is intended to say nothing about the quality of the studies not included. Indeed, perhaps because of competition for journal space, and because designs in this area are becoming classical, most reported experiments are well-controlled and methodologically sophisticated. We have, however, tried to include those articles which addressed themselves most successfully to questions of theoretical and factual significance.

The reader has a right to know about the reviewer's preconceptions. He approached the task with very considerable theoretical neutrality, and (it is finished now) nothing has been found to alter this. He has been impressed with "the factualists, congratulating them on their sanity, and the conceptualists, saluting them for their courage" as Boring has put it (17, p. 184). If the conceptualists appear to lose more often, it is because they are more liable to it; whether they will also win more often remains to be seen.

No obviously best way has been found to group and order this literature. It is arranged roughly in the order of events in the instrumental paradigm: drive, antecedent stimulus control, response variables and measures, rein-

¹ The survey of the literature pertaining to this review was concluded in May, 1954.

² The following abbreviations are used in this chapter: CR (conditioned response); CS (conditioned stimulus); sE_R (reaction potential); sH_R (habit strength); I_R (reactive inhibition); sI_R (conditioned inhibition); MMPI (Minnesota Multiphasic Personality Inventory); N (number of reinforcements); PGR (psychogalvanic response); r_G (fractional antedating goal reaction); S^R (primary reinforcer); S^r (secondary reinforcer); US (unconditioned stimulus).

forcement and extinction; then, a special section on latent learning, and finally one on theory.

DRIVE

Drive is ordinarily defined as an intervening variable or hypothetical construct linking an establishing operation (deprivation or aversive stimulation) with some property of response (appearance of consummatory or previously reinforced behavior; or a general rise in activity level). Some form of this construct is central to all modern learning theories, but with differences in the roles and properties assigned to it. The motivation construct currently carries a tremendous burden of explanation and this year's experimental yield reflects the great interest in it. But the power of this variable is more suspected than demonstrated, and the results of all the demonstration experiments, when done, may very well be a decreased assignment of variance to it as a causal variable. One can detect in this year's work a tendency to refer some of it to direct, external stimulus control, and to question the formulation of drive as an internal stimulus event. It is tempting to conclude that, however defined, drive is now correlated with all otherwise unaccounted-for variability in response characteristics. The general rules for the admissibility of a drive term are only very vaguely specified, with the result that the performativity of the reinforcement operation tends to be accepted as a sufficient condition for its introduction. There is an obvious danger in this procedure that drive states will grow *ad hoc* and *ad lib* without sufficient anchoring on the antecedent end. The drives underlying response strengthening by secondary reinforcers are particularly fuzzy in this regard: note that the same instances of the same operations can create simultaneously the secondary drive and its reducer, a secondary reinforcer, in Hull (53, p. 6). We may, in speaking of the aversive drives, be involved in a vast tautology which adds nothing to the account but does preserve a formula; at least, if these were all we had, it seems doubtful that the drive concept would ever have been invented. Additional problems which are treated this year include the questions of drive interaction and the specificity of the relation between a particular drive state and the response systems it controls.

Drive interaction.—Verplanck & Hayes (135) have shown conclusively that consummatory behavior with respect to food and water are intimately interrelated; deprivation of either results in a sharp reduction in intake of the other. Their food-deprived rats drank only .41 as much as when food was available, while food consumption during water-deprivation was only .57 of normal. This study suggests that as far as consummatory behavior is concerned, drive does not indiscriminately strengthen all available habits, and the effect may be quite the opposite. If one drive can also control noningestive responses acquired under another, the effect of combining drives should be to depress, not summatively strengthen, responses acquired under either. Partial confirmation is found in a study by Powloski (94) who showed that learning scores in a Y-discrimination maze for rats running under com-

bined hunger and thirst were between the scores for those running under either drive alone. The Verplanck and Hayes results complicate the interpretation of irrelevant incentive learning of the Spence-Lippitt (124) variety: according to the deprivation criterion, the hungry rat is thirsty when he sees the "irrelevant" water, but his drinking behavior is at low strength, making the drinking r_a^2 both more and less plausible. In addition, if thirst has "cues," the latter are being conditioned to the food going response, and may continue to do so on the test trial, when the animal is merely more thirsty.

Drive selectivity.—The foregoing would not be the case, according to Kendler's (59) selective principle, which states that only those drives which are reduced become connected with the rewarded response. Hull has considerably weakened his statement on the indiscriminate strengthening of all sH_R 's by total available drive. He now reads (53, p. 7): "At least some drive conditions tend partially to motivate into action habits which have been set up on the basis of different drive conditions." Which leaves us about where we were. Levine (68) shows that irrelevant drive states can acquire control over a response, although only after considerable practice. His experiment involved training hungry, thirsty, and both hungry and thirsty rats in a T-maze. The single drive groups ran to the relevant incentive; one hungry and thirsty group ran to food, the other to water. After considerable overtraining (90 reinforced trials) motivation conditions were switched for the single drive groups, and for all groups the opposite goal box was baited with the opposite incentive. According to Kendler's selective principle, learning the reversal should have been equally easy for all groups; actually, the groups running under combined drive were considerably retarded in learning the new habit, apparently because the irrelevant drive, though now relevant, strengthened the old response. In addition, Levine shows that rats can learn a spatial discrimination, pressing one panel when hungry and another when thirsty, with both responses reinforced by light avoidance; this, too, is evidence against the selectivity principle since neither drive was reduced, but still the rats acquired selective control.

It is such results which lead us to attribute stimulus properties to the appetitive drive states, but it should be noted that any reference to drive stimulus is either a figure of speech or, in the construct language, one step further from the data language than glossary definitions of stimulus tend to specify. "Stimulus" is a class concept, already one step removed from the physical events which are its instances. For the aversive drives, we distinguish between the drive-inducing stimulus and an additional drive-produced stimulus, but the latter is also an inference. If the probability of the inferred stimuli becomes doubtful, as in the case of the subhungers and indeed most of the appetitive drives, one is in trouble only if he has arbitrarily included stimulus events in his definition of drive. Campbell & Sheffield (22) question the adequacy of the drive-produced stimulus concept to account for what happened when they measured random activity of rats in tilt cages over

seven days. Once each day a brief period of marked environmental change was introduced: some humming exhaust fans were turned off and lights were turned on. For the first four days the animals were satiated; for the last three they were food-deprived. For all days, introducing the environmental change produced a significant rise in activity, with an adaptation effect setting in over the four satiation days. This trend was sharply reversed when the deprivation period began, producing a significant rise with increasing hours of deprivation, but only during the external stimulus period. These authors conclude that the effect of hunger is not to produce an internal stimulus to activity, but rather to reduce thresholds of response to external stimuli. Seward, Levy & Handlon (108) hypothesized that the difference between habit versus cognition learning can be tested by observing the behavior of satiated rats in a situation in which they had previously been reinforced under drive. If habit strengths have been created, the previously strengthened response should tend to persist, activated by other, residual drives; but if cognitions have been acquired, no differential response tendencies should appear in the absence of demand for the incentives. Their experimental results favored the habit strength alternative: rats forced to water on one side of a choice-box when thirsty, and to water on the opposite side when not thirsty, showed a significant tendency to persist making the turn which had previously led to drinking, even when they were satiated for water. These results seem clear enough; however, it is not clear that they constitute a test of divergent hypotheses because of the indeterminacy of Hull's current statement on the drive specificity issue.

Exploratory drive.—In three studies (81, 82, 83) Montgomery convincingly continues his argument that the exploratory tendency is a primary drive, aroused by novel external stimuli. Exploratory behavior has previously been "derived" as an effect of the general rise in activity level produced by some other drive. But Montgomery (81), using female rats, shows that concurrently hungry or thirsty subjects exhibit less exploratory behavior in a Y-maze than do satiated ones. In addition, this study shows a 68 per cent frequency of entry into three unlike arms in succession, which is far above chance; exploration is "systematic." This susceptibility of exploratory behavior to external (choice-point) stimulation makes it appear unlike an effect of a general (internally stimulated) activity drive. Thompson (133) has generally confirmed Montgomery's observations, but he found that with male rats food deprivation increases exploratory activity, while with females, exploratory activity rises from 0 to 24 hr. deprivation, then falls, at 48 hr., to a point below the satiated group.

Montgomery (82) also showed that activity deprivation does not affect exploratory behavior, as it should if the latter is motivated by a general activity drive. One group of male rats lived in rather small cages, while another had free access to activity wheels. Exploratory behaviors in the Y-maze measured on the first, third, and eighth days of such a regimen were indistinguishable for the two groups, both in amount and in orderliness. The

"deprivation" operation appears to have been successful, for the deprived animals mostly slept when not out exploring while those with the wheels did considerable running.

It should follow that novel stimuli can act as reinforcers. Montgomery confirmed this (83) by showing that rats will choose the arm of a Y-maze that leads on to a Dashiell maze in preference to a blind alley; however, such reinforcement does not act to satiate the exploratory drive, but, as judged by continuing choice of the maze side, to increase or at least maintain it. This exploratory drive phenomenon is similar to the manipulation motive as described by Harlow & McClearn (47). Their rhesus monkeys learned to pull certain colored bolts out of a board, avoiding the others which were not removable, apparently because the removable bolts could be played with. This "drive" showed no signs of satiation over repeated trials. These studies suggest that what we have here is a class of reinforcers (novel and manipulable things) that do not depend for their effect upon any drive-inducing operation, other than presentation of the stimulus itself, or upon drive reduction in any common (nontrivial) sense; at the same time, they are not "secondary." In short, if we call them "drives" it is because response-strengthening occurs, and it occurs despite other differences from the usual appetites. Glanzer (38, 39) proposes a stimulus mechanism to account for spontaneous alternation phenomena. He shows that if the stimulus objects characterizing a choice point in an unbaited T-maze are exchanged side-for-side between trials, hungry rats repeat turns; but this means alternating stimuli. If the rats are detained in the presence of the stimuli of their last choice, and choice point characteristics remain constant between trials, spontaneous alternation increases. Both of these results contradict the response-oriented I_R^2 hypothesis. Glanzer proposes (39) a stimulus-satiation principle which is rather like sI_R^2 but substitutes a reference to the stimulus in place of the response. Zeaman & Angell (148) gave rats either 2 or 10 forced 90° turns just before testing their free choice preferences in a maze with four alternatives arranged spoke-fashion over 180°. Avoiding the path to which they were forced, the rats chose with increasing frequency increasingly distant alleys. The authors point out that this finding is compatible with either the stimulus-novelty or I_R hypothesis.

Avoidance learning.—In the classical avoidance-learning experiment, an animal, by learning some response to a stimulus (CS^2) which has regularly preceded pain can "avoid" the painful stimulus (US^2) itself. There appear to be at least three separate *S-R* interpretations of this phenomenon. In the Hull (52, Chapter 6) and Miller (77, 78) formulations, the instrumental avoidance response is strengthened by fear drive reduction. Fear is an acquired drive whose acquisition in turn requires drive reduction: the termination of the US is assumed to strengthen the connection between the CS and the fear (now a response) which the US elicits. Mowrer (84, Chapters 9 and 10), without questioning the necessity for drive reduction in the acquisition of the instrumental avoidance response, argues that the drive itself is not ac-

quired by drive reduction, but that instead contiguity principles are sufficient, according to his two-factor theory, to give the CS elicitive control over the aversive drive which the US arouses. According to him, the Hull-Miller drive-reduction account of secondary drive acquisition generates erroneous predictions. One of these, tested by Mowrer & Solomon (88), is that a CS which is followed by a shock of brief duration will, because its termination (reduction of primary drive) involves less delay of reinforcement, elicit a stronger conditioned fear than will a CS followed by a longer shock, and that a shock which terminates abruptly will condition more fear to the CS than one which terminates gradually [Hull specifies abruptness of drive reduction as a drive conditioning parameter (52, p. 6)]. Neither of these predictions was verified in an experimental test. Also, Mowrer & Aiken (85) tested the effect of varying the temporal relations between onset and offset of both CS and US. When CS offset and US onset are simultaneous, maximum fear effects are elicited by the CS. Simultaneous CS-US onset is less effective, and simultaneous offset is least, although this last places the drive reduction closest in time to the CS, which, if drive reduction strengthens fear, should have led to greatest conditioning.

Dinsmoor (29), following Skinner (115, Chapter 11) and Schoenfeld (105), takes the step beyond Mowrer and convincingly argues that all of the avoidance phenomena can be accounted for in terms of direct aversive stimulus (CS and US) control, without reference to drive. This is the argument that suggests that if there were no appetitive drives, but aversive stimuli only, the drive construct might never have been invented. In both avoidance and escape learning the termination of an aversive stimulus is the reinforcing event (the event with which response strength is correlated). In escape training the reinforcer is primary; in avoidance, accepting the usual definition and the usual procedures, it is secondary. The CS becomes aversive "in its own right." Any response which terminates it will be strengthened as a direct consequence, and the "forward pointing" reference to the US in the term avoidance is misleading. [Wickens & Platt (139) show that the rate of acquisition of an instrumental avoidance response was accelerated when the response terminated the CS, as compared with a condition in which it persisted briefly after the response was made.] This formulation permits collateral conditioning of respondents (anxiety) as an emotional epiphenomenon with its own aversive properties (115) but not as a necessary stage between the CS and response.

Whatever one's opinion regarding the functions of stimuli antecedent to avoidance responses, stimuli may be produced by the animal's own behavior. Sidman (109) regularly shocked rats at varying intervals ($S-S$) without any warning signals. The animals could delay the next shock for various intervals ($R-S$) by pressing a bar. High rates of responding were generated, rising as the intervals $S-S$ and $R-S$ became shorter, except that rates fell rapidly when the latter became very short (so that an immediate response-punishment contingency was established). The cue here seems to be in the

animal's own behavior. Nonpressing behavior is most frequently correlated with shock; the stimulus properties of any nonpressing behavior, therefore, acquire aversive properties which can be avoided by pressing, which strengthens pressing. Of course, anyone who wants to say that nonpressing generates anxiety which can be reduced by pressing may do so, but the stimulus account appears sufficient.

Kalish (56), defining fear as a drive with cue and response properties, completely separated the drive establishing phase from the instrumental learning phase of the usual avoidance paradigm in order to discover the parameters of drive strength as revealed in the second phase. Latencies over successive trials in a hurdle jumping response were inversely related to the number of previous CS-shock pairings and directly related to the number of prior CS-alone ("extinction") presentations. Kamin (57), using dogs, tested CS-US interval variations on avoidance learning. Shorter intervals were associated with more rapid acquisition of the avoidance response, with more resistance to extinction, and with shorter latencies. It is these commonalities between parameters of classically conditioned observable responses and the drive variable that lead, in the Hull-Miller formulations, to calling fear a response.

Solomon & Wynne (120) report a study using dogs and a suitably modified Mowrer-Miller (87) shuttlebox apparatus. The CS was light-off, the US, presented 10 sec. later, was electric shock. An analysis of individual curves shows that escape latencies do not shorten much after the first few trials, nor does the response drift closer and closer to the shock and gradually pass before it into the avoidance condition. Instead, the first avoidance latencies suddenly begin in the vicinity of 5 sec. after CS-onset. The median latency difference between the last escape and next (avoidance) trial is 9.1 sec.; nearly equal to the CS-US interval. Control is abruptly passed from one stimulus to the other. These authors hypothesize that in escape training the animal is acquiring emotional responses to the CS via CS-US contiguity. When the CS acquires sufficient elicitive control over the emotional responses, the total stimulus pattern is sufficiently like the escape pattern to lead directly to the avoidance response with short initial latency. Significantly for Dinsmoor's argument (29), these experimenters observed CS-avoidance response latencies that were too short to include the mediating respondent stage (p. 17), a point which seems not to have impressed them at the time, but see below.

Avoidance learning is remarkably resistant to extinction, under some circumstances. "Extinction" procedures involve discontinuing the presentation of the US. Kamin (57) noted in his dogs that if the avoidance response actually terminated the CS, it showed no signs of extinction at all during 100 trials, while in animals whose latencies exceeded the duration of the CS, extinction occurred in about 50 trials. He attributes the difference to the extinction of the mediating conditioned emotional response in the nonterminating group. A suggestion as to why it should extinguish in one case and not in

the other comes from Solomon, Kamin & Wynne (119), who observed a similar resistance to extinction of the avoidance response. They became involved in what threatened to be a struggle to the death with one dog which, after being shocked only 11 times in the presence of a CS, acquired a jumping response to it that finally extinguished after 647 trials during which he was variously shocked for jumping, then imprisoned on the erstwhile shock side in the presence of the CS by a glass barrier which he at first jumped into, smashing his head against the glass ("He drew back and was fairly quiet. . ."). This apparently did it. When ordinary extinction procedures were systematically tested on groups of dogs, they led, if anything, to shorter and shorter latencies; the glass-barrier technique extinguished only 2 of 9 dogs; shock after jumping, 3 of 13 dogs (some latencies actually shortened under this procedure—getting it over with?); and a combination of the two extinguished 14 of 16. These authors suggest that the avoidance response remains at high strength, not because it terminates the drive or emotional state aroused by the CS, but because it avoids this state. This accounts for the CS-avoidance response latencies that are too short to permit a mediating fear response. "Fear" now has an entirely new role. It is necessary for maintaining the avoidance response because it will recharge the CS's aversive properties whenever the animal delays too long; but, immediate response control remains with the CS, which is a point for Dinsmoor.

Drive as an activator.—Latency is an increasingly used measure of response strength. Cotton (27) has investigated the relationship between running time and hours of food deprivation. He concludes that the major effect of increased deprivation time is not to increase (stimulate) speed of running per se, but rather to decrease the occurrence of competing responses (retracing, face washing, scratching, etc.); the slope of the line relating latency asymptotes to hours deprivation is strikingly reduced when runs on which competing responses occurred are excluded. Analysis of individual rat's records reveals that some show no increase in running speed with increased drive, and some show a decrease. Finally, there was evidence of significant (nonrandom) day-to-day variation in running times, which remains unaccounted for even in terms of "behavioral oscillation."

Anxiety.—Anxiety may be conceptualized as a built-in, constantly available drive state which people bring with them in different amounts to various situations. So defined, the state may be psychometrically determined by an "anxiety scale." Other things being equal, psychometrically anxious subjects in a learning experiment should show greater conditionability than the nonanxious, although the logic of this prediction requires that anxiety be one of the drives that indiscriminately multiplies all available sH_R 's. Positive results have been reported by Taylor (129), Spence & Taylor (125) and others on simple eyelid conditioning. However, in more complex (choice) learning, nonanxious subjects have been found superior (80, 130). This result is explained as attributable to the indiscriminate multiplying of all responses in the anxious, including any competing responses which are stronger, initially, than the correct responses. Ramond (97) was able to control the num-

ber and strength of competing responses in a paired-associate, verbal task. His results significantly confirmed one prediction: anxious subjects had greater difficulty learning the initially weaker association. They did not, however, learn the initially stronger association more rapidly than the non-anxious, as they should have according to the same prediction system. Spence & Farber (123) predicted that anxious subjects would learn a simple stimulus discrimination faster than nonanxious, since, if drive multiplies sH_R 's to both positive and negative stimuli, the difference in competing sH_R 's between them should also be multiplied, with a larger multiplying drive factor for the anxious subjects. Two replications of an experimental test of this hypothesis using the eyelid CR² were negative, but, "while not conclusive, the results of these two experiments tend to be in agreement with the implications of our theory. . . ."

Deese and co-workers (28, 67) have shown that there is a significant interaction between psychometric anxiety and the anxiety-eliciting components of the learning situation. Although anxious subjects were consistently superior to nonanxious ones in learning nonsense syllables the differences between the two groups were insignificant if errors were unpunished. They became "strikingly large" when incorrect responses were followed by shock, not because the anxious learned faster, but because of a loss of efficiency by the nonanxious. These authors suggest that this effect may be due to some personality correlate of low anxiety, and that this group actually contained a high incidence of hystericals (or, I would suggest as more likely, psychopathic deviates in the MMPI² sense).

We might speculate on what will happen when we discover that other psychometric scores enter learning curves as growth or asymptote parameters, as no doubt could be shown, say, for the Stanford-Binet and the learning curve of calculus. Will this be a drive because it acts like a drive? The answer that anxiety scale scores are entered as drive factors because of information which relates them to clinical anxiety presumes that there is more evidence for such a relation than our clinical colleagues are really agreed upon. In addition, it is not so clear that psychometric anxiety does act like a drive. Drives have traditionally been associated with momentary response strength changes over time during which no additional response strengthening or weakening operations have occurred. The anxiety factor as an individual difference variable appears not to act this way.

DISCRIMINATION AND GENERALIZATION

It is frequently assumed that the differences among the acquisition laws of various theories have inevitable consequences for their stimulus laws, and many discrimination and generalization experiments have been, and still are being, performed in an attempt to explicate these differences. Even more experiments are reported as opposition-stoppers, in the sense of purporting to reveal total inability of the other camp to cope with the observed outcomes. The point of the dispute probably lies in the centralism of the expectancy theories and the peripheralism of the *S-R* reinforcement theories.

For *S-R* theories, discrimination is a refining out of physical properties by a process of responding and reinforcement in the presence of them. Reinforcement creates the response strength but it also gives antecedent stimuli control over the response. For the cognition theorist, stimuli are learned, so to speak, as such; the connection acquired in the learned element is between stimuli, a central event defined without reference to the response criterion and without reinforcement. In addition to the response and reinforcement issues, a separate issue of stimulus complexity has arisen. In general, the *S-R* reinforcement theorist has defined stimuli (but this is definition in use) as points on "pure" stimulus continua, while the expectancy theorist has tended to define the stimuli in his *S-S* laws in field, Gestalt or relational terms, a difference which is probably not necessarily entailed in the differences between acquisition laws. Studies on relational learning are important and nutritious, but it is not clear how they are related to other systematic positions.

Relational learning.—According to the usual *S-R* reinforcement formulation, choice and transposition behavior can be derived on the basis of the absolute excitatory and inhibitory response tendencies conditioned to the separate stimuli (121); generally the responses are broadly defined as "approach" and "avoidance." In the relational view (cognition theories), choice is based upon relationships between stimuli as elements in a configuration. In a nonexperimental paper, Nissen (90) traces the stages of complexity through which the absolute theory remains adequate. He shows that as long as the same stimulus elements recur from trial-to-trial, however patterned, discrimination behavior can be accounted for by approach-avoidance tendencies, but that the absolute stimulus view encounters real difficulty when transfer to previously unexperienced stimuli occurs, as it does in the conditional matching problem (if background 1 is present, match forms; if background 2, match colors).

In an experimental approach to this problem, Nissen, Levinson & Nichols (91) concurrently trained chimpanzees on two separate discrimination problems: in one the larger of two white plaques was positive, in the other it was the smaller of two black plaques. These discriminations can be made either on the basis of absolute or relative cues. To discover which cues the animals actually were using, they were tested on novel pairs made up of one plaque from each of the original problems. When faced with a choice between the positive stimulus of one problem and the negative stimulus of the other, consistency of choice of the positive stimulus declined from nearly 100 per cent in the original pairing to 68 per cent, the attrition presumably a result of the loss of the relational cue; but choice did not deteriorate to chance. The authors suggest that both relational and absolute cues govern choice behavior, and that accuracy will be reduced as either is varied. This method appears to be one of the better ways of answering the complex questions in this area.

Hunter (55) has shown the durability of separate excitatory stimulus

strength to elements taken out of their original relations. He first over-trained rats to jump to the smaller stimulus (B versus A) in a simple discrimination problem until they consistently transposed to a still smaller stimulus (C versus B) on test trials; i.e., they consistently avoided B in favor of C. But later when B and C each was paired with a neutral stimulus in a new discrimination problem, it was harder to establish an avoidance response to B than to C.

There is something mystifying about this controversy, at least as it relates to the larger *S-R* reinforcement versus expectancy issue. It is hard to see how the *S-R* reinforcement theorist can be embarrassed by any data on relational responding, or at least why he should permit himself to be. A stimulus may be defined as physical energy, a public, objective, measurable event. But these are properties of relations such as larger than, squareness, or indeed any state of affairs with which an experimenter can maintain a response-reinforcement contingency. He is committed only to showing that, having maintained such a contingency, this set of properties is lawfully related to occurrences of the response, which is no more mysterious than a lawful relation to red or C-sharp. Apparently a theoretical preconception has been imposed on what is properly an empirical matter. The *S-R* theorists [except Skinner (112)] seem to have arbitrarily restricted the usage of "stimulus" to a special set of punctate physical properties, probably because of the purity of the generalization continua which can be generated with them and the usefulness of these in the derivation of stimulus laws, but this need not be a logical restriction. If the restriction has been severe on the stimulus definition, though, the latitude has been great in defining the response. Bitterman & Wodinsky (16) and Thompson (132) have rightly commented on the oddity of defining an effector-class as "approach" and "avoidance." This appears, among other things, to include the stimulus in the definition of the response, and to make it thereby a part of what is learned. [See MacCorquodale & Meehl (72) for an extended discussion of this and the stimulus-complexity issues.]

Perceptual responses.—Another issue is whether there is more to discrimination learning than the instrumental response which reveals it. Bitterman *et al.* (10, 11, 12, 20) have investigated the effects of "nondifferential reinforcement" on subsequent stimulus discriminability. In this technique, rats are reinforced for jumping to both the to-be-discriminated cards. For a constant number of such prediscrimination reinforcements, discrimination is more rapid, as compared with a control group whose prediscrimination jumps were to a different pair of cards (10), but the effect of increasing amounts of such pretraining is to make discrimination increasingly difficult for both groups (11). It is hypothesized that although nondifferential reinforcement generally retards subsequent discrimination by a sort of attentional adaptation, this may be partially compensated if the experience is acquired on the same cards as used in discrimination. In another experiment (12), stimuli, which had been irrelevantly associated with punishment in a

prior discrimination problem, were subsequently themselves discriminated without retardation despite their previous association with punishment. These studies suggest that between contexts of use of stimuli something transfers which is not a differentially reinforced response. Taking another tack, Bitterman & McConnell (14) have shown that the acquisition of a discrimination-set transfers from one problem to the next; an initial difficulty difference between two problems disappeared when the animals had had previous experience with the problem.

For their parts, the *S-R* theorists show increasing evidence of movement in the direction of including some sort of perceptual response concept in their formulations. There are otherwise unaccounted-for increases in the rate of acquisition of discriminative responses to previously discriminated, as compared with novel stimuli. The proposed solution involves postulating a discriminative response to the stimuli, as such, in addition to and different from the instrumental goal-response. This prior discriminative response should carry over to any new situation involving those stimuli, and new learning involving them should proceed without a prior period of stimulus differentiation.

Bauer & Lawrence (6) propose an "acquired distinctiveness of cues" mechanism to account for an effect they observed in rats learning a black-white discrimination in a Y-maze. For one of their groups the choice-point and goal-box brightnesses were reversed, so that a black alley led to a white goal box, and vice versa. At first, as anticipated by generalization, these animals were led into the wrong alley at the choice point. But this tendency soon disappeared, at which point they learned the correct discrimination at about the same rate as animals whose choice-point goal-box brightnesses were similar. On subsequent reversal training to choose the other alley, i.e., cue and goal-box relations as they had been, the rates for the two groups were the same from the outset. Eninger (33) in a very similar study, substantiates these observations, emphasizing the abruptness of reversal learning once the prior habit is extinguished. Reid (98) compared reversal learning between rats originally just-trained and those overtrained in a simple black-white discrimination problem. Although the overtrained rats persisted in responding to the previously positive stimulus longer than the just-trained, reversal was strikingly more rapid than for the other group, once this tendency disappeared, and over-all trials to the reversal criterion were significantly fewer. Ammons (1) has surveyed the literature on "perception" and finds a multiplicity of usages which reduce operationally to a core concept of the "recognition response"; this, in turn, appears, at least to the perceiver, to be general with respect to the class "recognized object." The available evidence is spotty, but substantiates Ammons' contention that its strength covaries with the usual learning theory variables, such as reward, punishment, distribution of practice, and so forth.

The *S-R* explanation of these transfer effects substitutes a peripheral response for the alternative central-cognitive event. The viability and conviction of this concept will depend upon identifying reasonable loci of the

components and preferably in demonstrating their existence outside the exigency that created them. This problem is shared with the r_g construct, which will be discussed below.

Differential reinforcement.—In his latest formulation (53), Hull defined V (stimulus intensity dynamism) as a multiplicative component of sE_R^2 which relates the intensity of a conditioned stimulus directly to response strength. The evidential base for this postulate comes from classical conditioning studies, but its generality for the instrumental case has never been well-established, partly because of the inability to specify an underlying intensity continuum for such common stimuli as the bar in a Skinner-box, or the choice point in a maze. Perkins (93) trained two groups of rats on the bar pressing response, one in the continuous presence of a light of medium intensity, the other in the presence of an intermittent light, with reinforcement presented only to presses occurring when the light was on. He then measured the effect of altering the brightness of the light on latencies of the responses. For the group given differential reinforcement the stimulus intensity dynamism seemed to hold: latencies were longer to a dimmer light and shorter to a brighter one. But for the group with light continuously present, it did not hold: latencies were the same to all brightnesses. Perkins concluded that stimulus intensity dynamism is not a general law, and that its effect can be derived from other Hullian principles. He argues that classical conditioning always involves differential reinforcement, with the CS as the positive stimulus and its absence the negative stimulus. This generates a gradient of extinction starting at zero value of the CS and extending downward across the stimulus continuum on which the positive stimulus, with its generalization gradient, lies. The interaction of the two gradients permits the derivation of the stimulus dynamism effect guaranteed by the classical conditioning procedure, but to be expected in instrumental conditioning only when the response has occurred in the absence of the stimulus and not been reinforced. Logan (69) independently proposed a very similar derivation, and shows that conclusions testably different from those based upon V can be deduced by it. However, contradictory findings are reported by Green (42), who evoked the dynamism effect without prior discrimination training. Using pigeons and the pecking response, he reinforced responding to a key illuminated with a small spot of light. During extinction, increasing the size of the spot increased the total number of responses emitted. These results agree with similar data reported by Skinner (113) in which color was the variable altered during extinction.

Wortz & Bitterman (146) have shown that the presence of an irrelevant stimulus property (inconsistently correlated with reinforcement and non-reinforcement) retards discrimination learning; so, apparently the mere presence of a stimulus at the time of reinforcement gives it some control over the response, although the Perkins study failed to show it.

Generalization.—Wickens, Schroder & Snide (140) and Grant & Schiller (41) point out that, although the primary stimulus generalization gradient is extensively used in nearly all learning theories, empirical evidence as to

its exact shape is "very confused." Unfortunately, the results of their experiments are about as different as they could well be. In both of them the PGR² was conditioned, to a tone (140) and to a visual stimulus (41); in both of them gradients were plotted throughout extinction presentations of the test stimuli. Wickens *et al.* found the first-test gradient to be flat to stimuli 25 to 50 jnd's away from the training CS; a bell-shaped gradient emerged with successive presentations of the test CS. Grant and Schiller found a convex upward gradient on the first test, which disappeared with successive presentations. In view of the uncertainty as to the empirical state of affairs, in this area, miniature systematizing and the deduction of precise mathematical predictions on the basis of generalization phenomena seems premature.

Classical conditioning.—Two studies of stimulus contiguity parameters in classical conditioning were reported. McAllister (75, 76) reopened the question of optimal interstimulus interval, which most studies miraculously agree in placing at about 500 msec. Using the conditioned eyelid CR, an auditory CS and a delay procedure, he discovered the optimal interval to be 250 msec., although 450 msec. was not reliably lower. Shifting the CS-US interval length away from an optimal value used in initial training led to a reduction in percentage of CR's. Wilcott (142), contrary to earlier findings, failed to find evidence for subthreshold conditioning.

Sensory preconditioning.—Bitterman, Reed & Kubala (15) demonstrated sensory preconditioning by exposing their subjects to a light₁—light₂ sequence. A PGR was then conditioned to light₂. Test trials to light₁ showed slightly greater amplitude of PGR to it than to CS light₂, and separate extinction curves to the two lights presented singly showed equivalent cumulative amplitudes to extinction. That this result is not explainable as simple generalization is shown by a suitable control condition. Sensory preconditioning has always strained *S-R* reinforcement theories. Positive results can be derived from them by assuming that in the preconditioning phase a common response is conditioned to the two stimuli. Any response subsequently conditioned to one of them will then be elicitable by the other via the mediating stimulus properties of the common response. This derivation is criticized because of the vagueness of the *R*-term and reinforcement source. However, results consistent with it are provided by Silver & Meyer (110) who show that when *S*₁—*S*₂ are serially presented in preconditioning, a response subsequently conditioned to the second will transfer to the first more than from the first to the second; so, if there is a mediating response, it is stronger, as it should be, after forward conditioning. Cognitive theories are more comfortable with preconditioning phenomena, and it is in these terms that Bitterman *et al.* interpret their data; they urge, also, that preconditioning is not as weak and unstable as is frequently assumed.

TRANSFER OF TRAINING

In a sense, as Smedlund (117) points out, "transfer" is a basic problem

in all learning experiments. Situations, in or out of the laboratory, seldom exactly reduplicate themselves. Their systematic variation in the laboratory leads to results that are variously called "generalization" or "transfer," depending mostly on a naming tradition that has grown up about classical differences in experimental design. Transfer designs have been particularly rich sources of information about temporal variables and serial interaction phenomena, and, although it would be quite improper to imply that there is no "theory" of transfer, its variables are remarkable for their strongly empirical rather than hypothetical nature. The research this year continues this tradition.

Motor learning.—Duncan & Underwood (30) investigated retention and transfer over a 14-month interval, as compared with a 24-hr. interval. Task I was a perceptual-motor, paired associates problem, involving lever positioning to colored-light stimuli; Task II involved different pairings of the same lights and lever movements. Transfer between Task I and Task II was positive. Measured 24 hr. later, there was no evidence of proactive inhibition by Task I on the retention of Task II; this is consistent with the usual finding that proactive inhibition ordinarily occurs when there is negative transfer from first to second task. However, when the subjects were retested 14 months later, evidence of "latent" proactive inhibition appeared, as an inverse relation between level of mastery during learning on Task I and relearning on Task II. The authors hypothesize that proactive inhibition increases with the retention interval, and is not restricted to tasks between which transfer is negative; it may be as important a factor in "forgetting" as retroactive inhibition. Atwater (3) also presents evidence of a positive relation between proactive inhibition in relearning a second task and level of mastery of a prior task, where original learning of the second task was facilitated by the previous learning.

Based on a theory of transfer proposed by Lewis (unpublished), Barch (5) argues that the identification of the factors that remain relatively unaffected in the shift from first to second task will improve predictions of amount and direction of transfer effects between them. Thus, some of the learning in the first task involves appropriate responses to general and unchanging features of the situation and some involves inhibiting of generally inappropriate and irrelevant responses. (These appear to be similar to the "general principles" and "work habits" of earlier concern.) The stronger these tendencies are, the greater is the transfer, or lesser the interference, to be expected to the second task. Barch attempted to control them by imposing different levels of difficulty on Task I in a Two-Hand Coordinator problem. Task difficulty was varied by using different sized targets. Task II involved a reversal of the controls and a shift to the more difficult problem. The groups originally trained on the more difficult task were superior throughout, as predicted.

Solomon & Coles (118) used a transfer design to investigate whether imitation responses learned by hunger-motivated rats in a T-maze would

generalize to a new drive and a new situation. Such results have relevance for questions of generalized imitative tendencies, and are related to the notion of functional autonomy of motives. Miller & Dollard (79) had shown that when imitation was acquired under hunger in a T-maze, the transfer to a thirst drive was nearly complete; however, these drives are not wholly independent. Solomon & Coles used, as the second task, an avoidance problem in which the animals could avoid shock by leaping a hurdle into a second compartment at the sound of a buzzer. On the other side of a glass divider was a leader rat, already trained in the avoidance response. If the follower rats trained to imitate in the maze, had transferred this tendency to the avoidance situation, they would never have been shocked at all. Under these conditions, however, the imitative response did not generalize; the course of learning of the maze-follower rats was not different from that of a control group. The test situation here, of course, elicits behavior of an emotional sort which is incompatible with imitative behavior and its precurent members, such as "watching" behavior. If the follower rats had imitated, it could not be said that they were doing so under a new drive, since they would have avoided the shock from the outset. It is possible that once the aversive shock is experienced by the follower rats, the interference behavior becomes conditioned, with the consequent disappearance of imitative behavior.

Verbal learning.—Porter & Duncan (95) and Harcum (46) present evidence which contradicts the old rule of thumb that negative transfer or interference is more likely when different responses must be learned successively to the same stimuli. Porter and Duncan observed clear evidence of positive transfer in the learning of two paired-associate, adjective lists with identical stimuli but completely different responses. However, if the second list was prepared by re-pairing the first list stimuli and responses, negative transfer was observed. These authors suggest that this may be due to a compounding of interference effects caused by backward associations from the first list's being revived during second list learning. The first of these effects is somewhat difficult to account for on *S-R* principles, and is similar to the results of some of the discrimination studies reported above in suggesting that during original learning some stimulus-associated process other than the observed and reinforced response is taking place.

Young & Underwood (147) on the other hand, cast doubt on the other old rule of thumb that positive transfer occurs when old responses are learned to new stimuli. They hypothesized that this effect may be attributable to response differentiation which takes place, as in the learning of paired-associate verbal material, in first-list learning, and that this is what transfers. If so, transfer should be minimal when intralist response similarity is minimal, i.e., first list learning would be less slowed down by response generalization. It should also be minimized by predifferentiation training on the responses before the first list is learned. Their experimental test of this hypothesis showed that predifferentiation training did reduce apparent transfer, but the difference was not significant; they loaded the dice against themselves, though, by using dissimilar responses. Their design also showed that

the amount of transfer, with or without predifferentiation, was not reduced when the responses were dissimilar from first to second task. They, therefore, conclude that the *A-B C-B* paradigm actually is not the paradigm for positive transfer.

The effects of different methods of training in the first task on transfer to a second task were investigated by Hilgard, Irvine & Whipple (50). The first task involved learning solutions to two of Katona's card tricks (58) in which the subject is required to arrange a deck of cards so that when dealt in a prescribed manner they appear in a special order. One group simply memorized the solutions, while a comparable group was first shown a formula by which the ordering could be figured out. Task II involved retention tests on the previously learned tricks plus a simple transposition (substituting odd and even for the previously learned red and black order), and three additional tricks which could be solved readily by the formula, but not by direct transfer of rote memorization. Retention of the two groups was about equal; the formula group had a slight advantage in the simple transposition problems, and a significant advantage in the completely new problems. Their superiority, however, was only relative; their absolute skill on the new problems was surprisingly low. Apparently they used the formula to derive a sequence which they then memorized, so that little skill in using the formula was acquired. It should be pointed out that this evidence does not show that "understanding" is superior to "rote memory," but rather that memorizing a principle leads to more transfer than memorizing a solution.

REACTIVE INHIBITION

Hull's reactive inhibition I_R and conditioned inhibition sI_R constructs enjoy some of the more favorable postulate-theorem ratios in his system. These constructs have been applied to a wide variety of experimental phenomena: reminiscence, spontaneous alternation, massed versus distributed learning, spontaneous recovery, and extinction rates. This year's experiments have been generally unfavorable to the formulation, and their discussants tend to reassign the effects attributed to these constructs to simple external stimulus control (38, 39, 51, 103).

Two studies (2, 106) refer directly to a previous investigation by Kimble (63) in which he analyzed the results of an inverted alphabet printing task under spaced and massed conditions into two components. The reminiscence effect, measured by the increment between last prerest and first postrest trials for a massed learning group, is assigned to I_R ; the difference between the first postrest trial for the massed group and the same ordinal trial for the spaced group is assigned to sI_R . The reinforcement for sI_R comes from "resting" responses. Schucker, Stevens & Ellis (106) reasoned that (a) rehearsal effects, not dissipation of I_R , might be taking place in intertrial periods for the spaced group, and (b) the spaced learners might have an advantage in knowledge of results after each trial. They repeated Kimble's study, controlling (a) by having all subjects look at photographs in intertrial intervals, and (b) by informing all subjects of results. In addition, since massing de-

presses acquisition, fewer correct responses have occurred for that condition before the comparison trial. These were equated in the present study. The results showed the reminiscence effect: postrest performance did improve for the massed learners, but no conditioned inhibition was observed. The improvement brought the massed learners up to the spaced learners' curve. Since absolute comparisons showed these spaced learners to average 4-6 letters fewer per trial than Kimble's, the authors feel increased confidence in their rehearsal hypothesis. Archer (2) corrected statistically for the difference in prior total correct responses between spaced and massed learners in the alphabet task, and showed that the massed learners were superior to the spaced learners. Uncorrected, the massed learners' scores showed a reminiscence effect, but as in the previous study, no permanent decrement sI_R . These studies suggest that work-decrement phenomena occur, but that their conditionability is in doubt.

Underwood (134) points out that it isn't clear that retention of serial nonsense material, anyway, is inferior after massed practice to a criterion, citing contradictory evidence. Although trials required to reach a criterion decreased sharply as the intertrial interval increased from 2 and 60 sec. (showing the advantage of spacing), recall after 24 and 48 hr. was superior after massed practice. Underwood suggests that there may be an interaction between the subjects' sophistication in such learning (his subjects were naive) and optimal spacing.

In an experiment by Holland (51), rats failed to show even reminiscence after rest following massed practice in a response choice situation. This result was predicted on the basis of response-produced stimulus novelty introduced by the rest interval itself. In massed prerest training each response occurs in the context of stimulus traces from previous responding. These are lacking in the early trials after rest, hence there should be an initial loss after rest. The results supported this prediction, but the loss only approached significance. Rapid recovery, also predicted on the basis of reinstatement of response-produced stimuli, was observed. A spaced group, switched over to massed responding, showed an initial slowing down through the first few massed trials, due presumably to the addition of, for them, novel stimulus traces. In a theoretical paper Saltz (103) presents a set of postulates which permit, without a work-decrement construct, the derivation of a diverse set of phenomena including reminiscence. He, too, proposes an analysis in terms of response-produced stimulus traces.

Rockway (102) investigated bilateral reminiscence in a pursuit rotor task. Subjects were given varying amounts of training with the preferred hand, followed by rest, followed by practice with the nonpreferred hand. Reminiscence to the nonpreferred hand did occur. Rockway argues that the definition of I_R implies an inhibitory effect localized in the responding effectors, leaving the present phenomena unaccounted for.

In the above study, reminiscence was found to be an increasing function of amount of prerest work, although the usual finding is that reminiscence is independent of effortfulness of the responses generating it (8, 9, 32, 49).

Ellis (31) points out that this is not inconsistent with I_R theory, which asserts that resting responses will occur when I_R reaches a threshold, which is the same for all responses however effortful. The rate at which the general threshold is approached will be determined by rate of responding and effortfulness, but once at the threshold, total accumulated I_R should be the same for all responses, and reminiscence after a rest will not vary as a function of response effortfulness. When the threshold is reached, cycles of starting-stopping will be generated, with more rapid cycles characterizing either rapid or effortful responding. These constitute sI_R trials and account for greater sI_R following effortful responding. However, Maatsch, Adelman & Denny (70) did not observe differences in rates of rats pressing 5, 40, and 80 gm. bars, nor in rates or total responses emitted in extinction on them. There was no evidence of "resting" behavior for the 80 gm. group, nor more rapid accumulation of sI_R (more rapid extinction) for effortful responses. Apparently, if rats are reinforced for pressing heavy bars, that is what they learn to press, and extinction seems unaffected by it.

THE LEARNING CURVE

According to the Hullian formulation and conventional methods of plotting the course of learning, habit strength increases as a positive growth function of the number of trials; learning is a gradual process. But Skinner (112) has shown that maximum rates of bar pressing follow a single reinforcement (but not always the first), and Guthrie has always insisted that learning is complete in one trial. Voeks (137), in an experimental test of the Hull versus Guthrie positions, observed the growth of a conditioned eyeblink under very carefully controlled stimulus conditions. She made them maximally similar from trial to trial in order to minimize sources of Hullian sE_R depressors, such as afferent interaction, I_R , etc. She concludes that, by eliminating such factors, "one eliminates also virtually all evidence suggesting a gradual increase of sE_R with N " (p. 147). For individual curves, probability of response jumped abruptly from 0 to near 100 per cent, while amplitudes increased in successive blocks of elicitations in only about half her subjects. On the other hand, half her subjects showed perfect jumpwise (Guthrie) curves, and the rest had only a few dips. Voeks shows how the group curve of her one-trial learners resembles the conventional growth function, not because each individual's responses are gradually strengthening, but because they are entering the curve full blown on different trials. Hayes (48) makes the same point by showing how families of individual curves of several different function forms combine to produce either sigmoid or negatively accelerated group curves. Especially revealing is a plot of a negatively accelerated group curve superimposed on a sampling of the curves of its component individuals, which uniformly show sudden "insightful" learning. He proposes that an average plot which more closely resembles its components can be made by displacing each individual's curve horizontally until their final (asymptote or criterion) points coincide. In this fashion the course of learning in the

vicinity of the criterion is revealed, and where it is sudden for most subjects the group curve will reveal this property.

REINFORCEMENT

The word "reinforcement" will be used here in its entirely neutral sense, referring only to an increase in some index of response strength as the result of maintaining a contingency between occurrences of the response and its consequences. The consequences that satisfy this requirement, i.e., that strengthen, are called reinforcing stimuli, or more simply, reinforcers (here abbreviated as S^R for primary, S^s for secondary reinforcers). Whether they are necessary for learning (Hull versus Tolman) and if so whether they are so because they are also drive reducers (Hull versus Skinner) are separate questions. The interchangeable use of the terms "reinforcement," "reinforcer," and particularly "drive reduction" in experimental reports, besides being inexact, prejudices the account.

Parameter values of reinforcers.—For a reinforcer to reinforce, drive intensity has apparently only to be strong enough to arouse consummatory behavior. Variations in S^R values (weight, size, quality, etc.) on the other hand, do appear to affect acquisition and extinction curve asymptotes, although Maher & Wickens (73) failed to show differential rates of error reduction in a multiple-T maze situation for rats reinforced with 1 versus 5 pellets, and running time differences were only barely significant at the end of training. The "amount and quality of the S^R " variable is obviously multi-dimensional and incompletely defined. With increasingly sensitive techniques of control and measurement we learn that this variable is not all one; its components affect response measures in different ways. The desirability of conceptualizing S^R s as stimuli rather than drive-reducers is argued by Guttman (45) who tested four concentrations of sucrose (4, 8, 16, and 32 per cent) as S^R s of rats' bar pressing. Sucrose reinforcement permits separation of the effects of nutrition (drive reduction) from stimulus (visual size) and response (amount of consummatory behavior) aspects of the reinforcement operation. During initial acquisition under continuous reinforcement, time to condition goes up as concentration goes down, and the relation between concentration and rate of approach to asymptotic rate of responding is positive. This contradicts Hull's (53) view that the number of S^R presentations required to reach the response asymptote is constant. The asymptotic rates themselves increase with concentration up to 16 per cent, then decrease, perhaps as a result of the satiating effects of the 32 per cent concentration or, more probably, to the strengthening by it of competing behavior addressed to the feeding mechanism (they keep licking it). At least when the rats were reconditioned on a periodic schedule which permits rate measurement without the complicating effects of eating behavior, rate was a direct function of concentration. Guttman extrapolated the function relating rate and concentration to (about) zero values of rate to find the "reinforcement threshold" value for sucrose concentration. It is of the same order of magnitude as that independently established as the rat's sucrose taste threshold! If

S^R s are found to reinforce according to well-known stimulus laws of intensity, threshold, and reflex magnitude, we are less likely to equate them solely with drive reduction.

Carper (23) investigated the interaction between "quality" of S^R (saccharin versus glucose) and drive on the rat's bar pressing behavior. The effects of the two incentives were different, somewhat puzzling, and the appeal to drive reduction was at best not clarifying. Both strengthen barpressing, even when the animal is satiated. The rate of pressing is about the same under each, and increasing general food deprivation from 0 to 12 hr. does not significantly increase it. However, if a specific caloric subhunger is superimposed on the drive schedules, rate of response to glucose increases, while rate to saccharin decreases. Most interesting is the fact that there is no difference in resistance to extinction after saccharin and glucose reinforcement; glucose has drive-reducing as well as secondary reinforcing (taste) properties, but it is not more effective against extinction than saccharin, which has no primary drive-reducing properties at all. Perhaps the significant property of reinforcers is the fact that the animal has strong consummatory behavior with respect to them, which can be raised by deprivation for some stimuli, but which for others like saccharin ("sweet things") is high on an evolutionary-biological basis. In this event, saccharin would have a primary, or at least unconditioned, reinforcing value even though it is not nutritious. In another study Carper & Polliard (24) show that the intake of both saccharin and glucose increases with caloric deficiency. The notion that saccharin is not a secondary reinforcer is supported by the fact that saccharin intake does not decrease (extinguish) with time; it increases, at least through a 42 day interval.

Reinforcement schedules.—It is often remarked that in "real" life, one-for-one response-reinforcer correlations are the exception. Laboratory investigations have shown that, in general, partial reinforcement programs lead to increased resistance to extinction, as well as to different rates during extinction. In addition, momentary strength of free responses in orderly schedules, such as fixed-interval or ratio reinforcement, comes to match the momentary probability of reinforcement, indicating that various discriminations are set up on the basis of the schedule itself (112, 113). Skinner argued that the decreased rate of responding immediately following eating in fixed interval schedules is due to the fact that responding is never reinforced immediately following eating. Rate recovers with time since eating as a temporal function of the reinforcing stimulus. Ferster (37) has verified this on pigeons by means of a technique which effectively takes the bird out of the situation: if the lights go off, they don't peck. If this is done following a S^R presentation, the S^R apparently loses its temporal control, since turning the light on again is immediately followed by pecking, instead of the usual waiting. Ferster has also shown that the number of responses in an interval is a function of the number of responses emitted during the previous interval (also considered as a stimulus with temporal properties) since extended blackouts destroy response control from one interval to the next. Ordinarily, number of re-

sponses per interval is constant; response rate varies, therefore, with length of interval. See Wilson (143) for recent evidence on this point.

Wilson & Keller (144) have investigated another kind of scheduling in which responses emitted during an otherwise fixed interval served to extend the interval itself. S^R presentation was made contingent upon a rat's bar pressing following an interval of nonresponding. At all intervals of such "reinforced delaying," the rate of responding exceeded the required waiting period, although it did decrease as the interval was increased. These results show clearly that low rates ("latencies") of responding can be conditioned as such, even though, as subsequent extinction curves show, the response is by no means at low strength. The mechanism which permitted the animals to slow down their bar pressing was the evolution of idiosyncratic but fairly stable response chains intervening between a reinforcer and the next bar press which rode along with its strengthening.

The generalization that partial reinforcement generates greater response strength per reinforcement is based nearly exclusively on extinction evidence. Wike has shown (141) that it is also more difficult to condition a new response if it competes with a previously partially-reinforced one, due to the subjects' (rats again) greater "fixation" on the prior response. Here again the control is largely discriminative. Responding in the past has been reinforced following nonreinforced responses of the same form. The stimulus effects of these become the occasion for repeating the response. After continuous reinforcement, on the other hand, a failure of reinforcement alters the stimulus antecedents, leading to variability of the next response.

Secondary reinforcement.—A S' is a previously neutral stimulus which has acquired strengthening properties through association with a primary or already-established secondary reinforcer. The effect seems to be no longer in much doubt, although it is often used as a plausible but *ad hoc* inference to account for what is otherwise-unreinforced, cognitive-appearing learning. The fact that it involves "stimulus learning" gives Hull's secondary reinforcement corollary (53, p. 6) a most anomalous status in his theory, especially as it is corollary to a postulate that talks about response strengthening. Perhaps this was a sort of durable oversight. Skinner pointed out in 1936 (111) that stimuli which have functioned discriminatively for reinforced responses may be used as S' s in other situations. If this is true, and if only such stimuli become S' s, the acquisition laws for them need to specify more than other-reinforcer contiguity. Webb & Nolan (138) report further confirmation of the necessity for prior discriminative use of a secondary reinforcing stimulus. After rats had established a simple black-white discrimination they were shifted over to a T-maze in which one end box was black, the other white. Those rats then reinforced for turns to the goal box, which was the same brightness as their positive cue in the prior discrimination, learned faster than those trained to the previously negative cue. These authors trace the facilitative effect to the "state of secondary reinforcement" [sic] evoked by the presence of a positive discriminative stimulus. They also report that "Several unpublished experiments by the present authors fail

to reveal demonstrable secondary reinforcing effects of neutral cues associated with reward when these cues did not serve as discriminanda for the reward. . . . " This is a significant possibility toward the definition of what a S^r is and should be verified.

The magnitude or amount of the S^R with which a stimulus is appropriately paired might reasonably be a parameter in its acquisition of S^r properties. Lawson (66) found that, although presentation of the previously baited, but now empty, goal box at the end of a runway increased resistance to extinction of the rat's running response, the effect was not greater for animals previously fed four, as compared with only one, pellet there; also differential amounts of primary reward did not create differential rates of acquisition of a turning preference in a U-maze when the goal box was taken from the runway and located, empty, on one side of the maze. Greene (43), in a similar study, did show the differential effects of varying the size of food pellets in black versus white goal boxes on the secondary reinforcing properties of black versus white cues in a subsequently learned discrimination problem; rats previously fed large pellets in a black goal box learned to choose a black alley faster than those fed a small pellet there. The Lawson study should be regarded as only suggestive; he discarded an enormous number of rats for refusal to run, etc.

Since S^r s need not be consumed, or involve any apparent consummatory response, the relation between their effectiveness and maintenance schedules is an interesting question. A somewhat inconclusive answer is given in a report by Seward & Levy (107) who tested the efficacy of a S^r in satiated rats. Original preferences for differential endboxes were tested for satiated rats in a T-maze. The goal boxes were then used as discriminative stimuli (positive and negative) in a choice problem under thirst. When the animals were then satiated and returned to the original T-maze with the goal boxes back in place, a significant but asymmetrical shift in preferences was observed: animals watered in their originally preferred endbox greatly increased their frequency of turns to it, while animals watered in their nonpreferred endbox did not. This asymmetry is confusing, as is the fact that when they do occur shifts to the side of the S^r are abrupt, unlike usual reinforcement produced phenomena. In addition, some shifts of preference showed up on the first T-maze test trial after discrimination training, thereby resembling a latent-learning effect.

Another drive- S^r interaction was observed by Calvin, Bicknell & Sperling (21) who showed that the presentation of a stimulus previously paired with eating did not, as a S^r , reduce the hunger drive of rats as measured by amount of food eaten. If anything, hungry rats eat more in the presence of a food-based S^r , which suggests a drive-conditioning effect. Perhaps another way of putting it would be that the S^r exercises discriminative control over consummatory responses to the reinforcer with which it was paired.

Secondary reinforcement has been presumed to be ubiquitous and potent in molding human behavior. The Lamberts (65) observed that children's estimates of token sizes grew and declined as the tokens acquired and then

lost secondary-reinforcing properties. The subjects turned a crank nine times for a red token, which bought a chance to turn nine more times for a white one, which bought a gum drop. After five such sequences the white token (but not the red one) was judged, according to the children's adjustments of the size of a spot of light to equal it, to be significantly larger than it had appeared before the experiment. After three no-gumdrop extinction trials, the apparent size of the token dropped back to about preexperimental level. The criterion here, adjusting a spot of light, is an odd response measure, but its values are correlated with the operationally-adequate establishing procedures of S 's, and about all one can say is there it is. The proper response term, cranking, did strengthen and then decline.

You never can tell, as the Hawthorne studies showed, just what is going to reinforce human behavior. Reynolds & Adams (100) confirmed the previously observed (99) superiority of pursuit rotor performers who are rewarded for staying on target by hearing a click. The optimal relation seems to be one click for every half second continuous on-target time. This appears to be a pure feedback effect of some generality for the human reactor, but it is no easy problem for theory.

As Skinner points out (116) most verbal behavior is not primarily reinforced; demands and requests may be, but the accuracy of verbal reports requires that the reinforcers for them remain uncorrelated with the speaker's specific drive state. Since S 's are not primary drive reducers, they appear to fill the requirements, except that they must be generalized, that is, have a history of pairing with many different drives. The word "good" as a reinforcer is an example. Cohen *et al.* (26) showed conclusively with adult males that the frequency of sentences beginning with I or We could be significantly increased if the experimenter merely said "Good" in a "flat, unemotional" way after each occurrence of such a sentence. When the S was omitted, as in extinction, the frequency appeared not to fall, but when it was shifted to sentences beginning He or They, such sentences rose in frequency. Most of the subjects, surprisingly enough, were not aware of the special contingency between their behavior and the experimenter's "good."

EXTINCTION

Phenomenally, extinction is the other side of the coin from conditioning or acquisition; it is often used as an index of the strength of prior learning and as such has occurred as a term in most other sections of this review. However, extinction may be an object of investigation in its own right. Hull, for example, accounts for extinction in terms of the evocation and conditioning of inhibitory tendencies, I_R and sI_R , which have been discussed previously in another context. Gleitman, Nachmias & Neisser (40) have criticized this formulation at considerable length. They point to its empirical inadequacies (to account for subzero and latent extinction, which occur with no overt responses to generate inhibition), to its conceptual difficulties (the

status of a not-response in a response-reinforcement system), and to some of the paradoxical derivations it leads to (including, as they see it, the deductions that both learning and extinction are impossible!). Many of these objections appear to us to be valid; it remains to be seen whether these difficulties are real and insurmountable. An alternative way to view extinction is to make it a direct function of the stimulus alteration introduced by the withholding of the reinforcer; this constitutes a departure from the conditions obtaining during reinforcement, and the attrition to response strength is proportional to it. Continued nonreinforcement is "novel" to the extent that reinforcement characterized the acquisition phase. After intermittent reinforcement, for example, a failure of reinforcement is a less discriminable event and extinction is delayed (86, 114). Three current studies support this view (13, 19, 44). Grosslight, Hall & Murnin (44), in a verbal learning situation, found that subjects who had previously experienced sequences in which reinforced responses always followed unreinforced ones showed greater resistance to extinction than those whose schedules were so patterned that reinforcement always preceded nonreinforcement. Bitterman, Feddersen & Tyler (13) ran rats in a runway under intermittent schedules so that they were always reinforced in, say, a white goal box and never in a black one. Animals given subsequent extinction runs to the unreinforced-in box showed greater resistance to extinction than those extinguished exclusively to the previously positive box, in spite of the demonstrated sustaining effects of the latter as a *S'*. According to the novelty view, a failure of reinforcement in the previously reinforcer-correlated situation is discriminated more rapidly and leads to more rapid extinction. That the failure of reinforcement can function as a discriminative stimulus has been shown by Bullock & Smith (19). Every day for 10 days they reinforced the first 40 bar presses and obtained immediate extinction curves from rats. Repeated reconditioning-extinction leads to successively smaller extinction curves (to a limiting value of about 10). This again is primarily a discrimination problem, similar to the effect noted in interval or ratio reinforcement in which the presentation of a reinforcer becomes the occasion for lowered response rate.

Two studies [Brady, Stebbins & Galambos (18); Hunt, Jernberg & Otis (54)] have shown that an emotional response can be "extinguished," or at least made to disappear, without the usual repeated unreinforced elicitation of it. In both instances a conditioned emotional response (measured by the cessation of a food-reinforced, bar pressing response upon the presentation of a clicking noise previously paired with shock, and also by increased urination and defecation) was established in rats. Then the animals were subjected to convulsions outside the bar pressing situation and without involving the click CS. When the rats were returned to the bar pressing situation, the previous emotional response to the CS had disappeared. It might be argued that a large component of the response to the CS spilled out during the seizures, but this is hardly "nonreinforcement" and does not occur in the presence of the CS.

LATENT LEARNING

Total output in this area has declined in recent years, and it has changed in tone. We are less concerned now by the urge to show that the phenomenon occurs, which seems clearly established, than to discover under what conditions it occurs, and what significance it has for theory when it does occur. Casting at least some of the classical designs in the role of the crucial experiment has proved unrewarding, attributable partly to ambiguities in the designs themselves and partly to ambiguities in the theories the designs were once thought to test. Some of the least exploited designs are, however, most promising, as will be noted below. An evaluation of these designs and the literature of each may be found in (72, 131).

Wolpe (145), in a theoretical article, discusses a possible neurophysiology of latent learning. He says "It is rational to turn for an explanation of latent learning to the place where it occurs—the nervous system." True, but the fact is that what we turn to is not the nervous system, but a possible one, of which there must be many. We should think that a set of descriptive laws from this not-inaccessible domain might play an explanatory role in linking the terms of molar behavior laws, and certain findings could be crucial (e.g., that brain states induced repeatedly in succession could all be revived by induction of the first only, without motor output or additional sensory input). But at present our knowledge of such possibilities is crudely fixed by the same molar events they are invoked to explain. And, in the final analysis, to the extent that neural events make for differences in molar laws, these differences will still characterize the molar laws, and should be discoverable without the neural facts, although these would certainly speed things up, if we had them.

Irrelevant-incentive learning: strong competing drive.—Most previous studies in which rats were exposed to one incentive while motivated for another have revealed little or no latent learning, however Christie's results (25) were positive, a discrepancy which he attributed to the special preexperimental training in exploration and in deprivation he gave his subjects. This, he said, created a general tendency to perceive the environment. Vineberg (136) has confirmed several of Christie's findings, but objects to his explanation. After early experience in exploring a variable apparatus, his rats were given irrelevant-incentive training in a T-maze. Some were trained hungry; some thirsty. When motivations were switched, 8 of 10 hunger-trained rats went to water on the first thirsty trial, but only 2 of 10 thirst-trained rats switched to the food side. This is a common result of this kind of study. Vineberg points out that this asymmetry of effect argues against a general perception tendency; he suggests, instead, that the consequence of early training is largely emotional adaptation, to "experiments" presumably, which gives anticipatory goal responses a clearer field to operate in during the subsequent latent learning phase. The failure of adapted, thirst-trained rats to learn the location of food he attributes to their incapability (which seems too strong) of *releasing* dry food when thirsty, hence these are missing or too weak to mediate the opposite turning response when motivation is switched on the test

trial. So the test trial asymmetry is traced to an asymmetry in r_g learning under hunger and thirst. Kendler, Karasik & Schrier (60) have shown, however, that it is not necessary to acquire r_g s to the irrelevant incentive in order to produce switching behavior when motivational conditions are changed from hunger to thirst. They ran hungry rats in a T-maze with food in one endbox and nothing at all in the other. When motivation was switched to thirst, these rats ran to the empty goal box, not, obviously, because of any drinking r_g s attracting them into it, but because, these authors say, of the aversive properties for a thirsty animal of the strong eating r_g s conditioned to the food side. The effect was much less pronounced for animals switched from thirst to hunger. Now the needed asymmetry must lie in the relative aversive strengths of r_g 'ing food versus water for the unmotivated organism.

Several rather direct tests of this latent learning design have been applied to human learners. Bahrack has shown (4) that high motivation suppresses irrelevant stimulus learning in them, also. A group that was paid money for learning the names of geometric forms was significantly less able later to recognize the colors used in the forms than a group whose name-learning was not so rewarded. Stevenson (127) showed that three- to six-year-old children can learn the location of irrelevant incentives in a Y-maze, using forced trials and measuring position preferences. Success is related to age; actually the three-year-olds were not better than chance.

Irrelevant-incentive learning: no competing drive.—The frequency of positive results in latent learning when rats are exposed to an incentive with no competing drive is higher than in the preceding design. Kendler & Levine (61) ran three groups of satiated rats in a T-maze: one group ran to unbaited goal boxes; one to food on the right and water on the left; and for the last these incentive locations were reversed. All groups were then given motivated and reinforced runs to food on the right and water on the left. The group which had experienced the incentives in these locations in the satiated phase learned fastest and the group which had experienced them in the reversed locations learned the slowest. The significance of this study lies in showing that experiencing but not consuming an incentive in the "wrong" location retards subsequent reinforced learning; i.e., that during the latent phase the location of the incentive creates response tendencies that show up during later motivated runs. The alternative, but refuted, possibility is that the latent phase is merely an adaptation period which the presence (but not the location) of the reinforcers facilitates, thereby speeding up subsequent reinforced learning; it may be this, but in addition, the actual location of these "pacifiers" exerts its effect.

Free exploration studies: multiple-T maze.—In this design rats have traditionally been permitted to wander about in an unbaited maze, following which they are fed in the goal box. The test for latent learning is the number of cul entries made on the next run. Meehl & MacCorquodale (71) showed that one of the effects of free maze exploration itself, without any feeding experience at all, is an increasing tendency to stay out of the culs. We based

this on the behavior of adult rats in a 4-in. wide alley maze, tracing the cul-avoidance to the aversive consequences of turning around as opposed to unimpeded forward motion in the "correct" path. Kimball, Kimball & Weaver (62) observed free-exploration behavior in rats as well as test-trial behavior in multiple-T mazes. Preceding the test, animals were made thirsty and then given water directly in the goal box (three times). On the test run these animals made significantly fewer cul entries of significantly less depth in significantly less time than a naive control group. But would this have been the case even without the intervening drinking experience? Spontaneous end-to-end runs had increased in frequency as exploration exposure wore on, but so did frequency and depth of cul entries which confounds the interpretation of the test trial results. It is too bad these experimenters did not give one start-to-goal run just before the watering experience. Their rats were young and their alleys six inches wide, so cul entry might not have been so negatively reinforcing for them; perhaps the other effect, unimpeded forward running, accounts at the same time for the increased frequency of spontaneous "correct" runs during exploration, although how this would interact with the test conditions is not clear.

Free exploration: runway.—The above three classes of latent learning designs are not clearly definitive with respect to the *S-S* versus *S-R* reinforcement controversy. We have seen that response-entailed habits, with respect to the incentives or the maze itself, can be inferred to result from procedures involved in the latent phase, especially if given enough latitude with the use of *rgs*. However, even *rgs* appear not to be useful in accounting for the type of latent learning of which Strain (128) provides a current, careful example. His rats were given free exploration experience in a runway divided up into six differentiated compartments separated by swinging doors. These doors carried designs which were the only directional cues in the apparatus. Each rat was always inserted in the same compartment. Following this training the rats were placed directly in a detached replica of one of the end boxes and shocked there. Shortly they were reintroduced into the same runway compartment as before. Fifty-three out of 80 (significant) exited from this compartment in the direction away from the shock box; the closer the compartment of entry was to the shock compartment, the higher the frequency of departures the other way; rats that started toward the shock compartment tended to reverse direction before they got that far (which was unlike their preshock behavior); and total time spent in each compartment increased with its distance from the shock compartment. Extramaze cues, and the direction in which the rat was aimed at the time of insertion, were carefully controlled. Only the designs on the doors "told" the rats in which direction they were headed. It seems incredible that, since nothing was contingent upon it, the needed discriminations had been formed; but apparently such was the case. Any *rgs* here would be most vague as to topography; also, how would they be reinforced? To the argument that *rg* is a construct and is called a response because it obeys response laws, the answer is, of course, to ask for proof that this means more than that something was acquired. In the same

situation the expectancy theorist agrees that something was acquired but he disagrees that the something was a response. If this controversy has a point, it must be close to this. Behan (7) has shown that, given the goal response, "expectancy" can be derived as a corollary from Hull; but this depends again upon just how it is given.

Incidental learning.—This form of learning in man appears to be the analogue of latent learning in animal subjects. Typically, it is revealed by the ability of subjects to recall the stimulus properties of material experienced without instructions to learn, at least "about" them. Typically it is weaker, per unit of exposure time, than intentional learning. Saltzman, however, has shown (104) that if incidental learners are required to make a discriminative response to the stimuli, although not the one subsequently tested for, their learning is not significantly less than another group similarly trained but given additional instructions to learn. Neimark & Saltzman (89) showed that when stimulus presentation rates are rapid, there are no differences between intentional and incidental learning, while slow rates give the intentional learners a significant advantage. This area is related to the larger problem of the status of "instructions" as motivators and as antecedent stimuli, and how "instructions" relate to other nonverbal and more immediate antecedent stimulus operations.

Postman & Tuma (96) urge that incidental learning may take place without necessarily involving, or being reflected in, the same habits that intentional learners acquire in the same situation. Their subjects showed no evidence of having learned the locations of the words marking an alternative pathway through a mental maze, but they were able to recall significantly the stimuli that marked it.

THEORY

Although there were no major (book-sized) statements on learning theory this year, several significant papers focus primarily on problems of formulation itself. Estes & Burke (35) have presented the stimulus model for Estes' mathematical learning theory (34). It involves the notion of stimulus sampling on successive learning trials, with the dependent variable datum, probability of response, related to the probability of occurrence, on any trial, of the elements in the total possible stimulus situation. Connections between elements and response may be formed on an all-or-none basis, and for the stimulus model it is irrelevant whether they are formed by contiguity or reinforcement. Forms and parameters of the learning curve can be predicted for a given situation from knowledge of the sampling probabilities of the components of its stimulus complex, thus avoiding any necessity for a special postulate for the gradual growth of probability increment. Such knowledge can be got from other experimental analyses of the same situation. Estes & Straughan (36) have presented an experimental test of this theory which proves it capable of accounting for an otherwise puzzling effect noted in the Humphreys guessing situation: namely, that the asymptote of the rate of predicting an event matches the rate of occurrence of the predicted

event, although this does not, effect-wise, maximize the frequency of correct predictions. The theory assumes that to the effective stimuli acting at the time of choice there are connected the responses occasioned by the subsequent occurrence or nonoccurrence of the reinforcing event, making this response the terminal event on any one trial. It is shown that Estes' formulas, derived for other reasons, closely predict the actually observed asymptotes, as well as the rates of approach to them, and sequential effects within the curves for both groups and individuals. The fits are strikingly good and this approach promises to be extremely fruitful. Psychologists will have to look to their mathematics!

Spence (122) has restated his position with respect to mathematical theories of learning, emphasizing the unifying function of mathematically specified constructs and claiming emptiness for them as far as problems of locus (central or peripheral) or strengthening operations are concerned. This is certainly true for the class, but whether or not it is quite true for a given construct depends, it seems to us, on where one goes to get the values that are at once the number and the construct; that is, to the network of causal laws in the rest of the theory. If one starts with three low-level empirical functions relating N reinforcements to probability of response, latency, and resistance to extinction, sH_R may be interposed as the unifying construct, itself a function of N , with additional functions relating it to each of the dependent variable values. But this number is variable only with respect to the application of a certain class of strengthening operations to the occurrences of certain peripheral response events, and it relates to subsequent occurrences of them. It just isn't clear to what extent these specifications are wholly outside the number which is sH_R . sH_R is what-is-learned (not so blind an alley) in this system. If we are given merely the formula for it or if it really is just a number, we cannot apply it. Given the application and computation rules for it, it becomes useful, but also further, and nonmathematically, defined. What the rat learns may be characterized by a number, but this is not to say that he learns a number. Similar considerations apply to the locus problem for r_G and to Osgood's, to me, indiscriminately different "mediation hypothesis" which is treated at length in his recent book (92). Both of these have been characterized as inappropriate to the locus question. But if the S - R reinforcement theorist disowns his peripheralism, then the difference between him and the expectancy theorist becomes trivial. Koch (64) notes the inroads on Hull's peripheralism as his theory evolved, and points to the ensuing difficulties in the basic formulation.

An overview of this year's experimental literature suggests some generalizations on the impact of theory on the behavior of researchers. A careful reading of every article would surely lead to agreement with the author that the year's yield has not been definitively favorable to any theory, as such. In spite of this, there is a curious tendency for experimenters to trust their theories more than their facts as exemplified by a number of two- and even three-part studies whose early parts contradict theoretical prediction, leading

the experimenter to wonder what he did wrong. But can one "do things wrong," in this sense, in science? If the wording of a postulate set is not clear enough to tell in advance that the proposed experimental test is adequate, then there is something wrong with the postulates. To evaluate a test in terms of the congruence between it and the prediction, and to revise the test instead of the prediction base seems to be a peculiar reading of science and its method. Moreover, it impedes progress toward theoretical adequacy by preserving faulty or imprecise or indeterminate postulates. Theory (any theory, mind you) is potentially self-correcting; our theories will suffer unless we take fuller advantage of this possibility.

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VISION^{1,2}

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The role of visual perception in psychology (and the senses in general) has changed considerably in the last 25 years as systematic problems became defined predominantly in terms of behavior and conduct and less in terms of conscious experience. Sensation and perception have yielded, by and large, to learning and motivation as the chief testing grounds for competing theories of psychology. Many psychologists are nonetheless interested in discriminative capacities and characteristics of the visual system per se. The study of vision constitutes to some extent an independent discipline in its own right, investigated, however, by specialists in many fields. The vision psychologist, by investigating the functional characteristics of the visual system in terms of dependencies of discriminative responses on stimulus properties, generates perceptual constructs (or in subjective language, the perceived world) intervening between properties of the stimulus and other classes of behavior determinants. However, few theories of behavior have developed sufficient precision to realize the economy of rigorously defined perceptual constructs which, in a sense, act as the functional stimuli of behavior. This review reflects this state of affairs. The bulk of it concerns studies of visual capacities and characteristics per se, with only one section devoted to the topic of interaction of visual discrimination and other classes of variables influencing the discriminative index response. The review of necessity is selective. Many excellent studies on human engineering and other applied aspects of vision were excluded, and physiological-comparative problems of vision received far less coverage than they deserve.

BASIC VISUAL FUNCTIONS

Methodology.—In a paper on methodology of threshold measurement, Blackwell (22) makes a convincing case for his conclusion that visual theorists should test their theories on the basis of predictions other than the shape of the visual threshold curve. Very extensive data are needed to differentiate between the normal ogive and normal integral in log I in terms of critical

¹ The survey of the literature covered by this review was concluded in May, 1954.

² The following abbreviations and symbols are used in this chapter: S (subject); cff (critical flicker frequency); ERG (electroretinogram); C. I. E. (Commission Internationale d'Eclairage), also known in the United States as I. C. I. (International Commission on Illumination); LGN (lateral geniculate nucleus); GSR (galvanic skin response); RT (reaction time).

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curve fitting. Frequency of seeing curves are easily fitted by Poisson sums if there are no restrictions on the parameter (n) which represents the critical number of events and which determines the shape of the Poisson sum. However, Hecht *et al.* (90) and van der Velden (181) have assumed that variability of response reflects quantal fluctuations in the stimulus and have determined quantal number (n) for threshold in terms of the best fitting Poisson sum. Blackwell's analysis certainly invalidates this particular method of estimating the minimum number of quanta for vision. Blackwell (21) also criticizes the neural quantum theory of the difference limen proposed by Stevens *et al.* (176) in terms of the difficulties of differential curve fitting. His own extensive psychophysical data, obtained with visual stimuli, are not fitted by the rectilinear curve required by the neural quantum theory. A more complete report of Blackwell's very significant studies on methodology of psychophysical threshold measurement appeared recently in monographic form (23). Verplanck *et al.* (183) describes further investigations of the nonindependence of sequential psychophysical responses which was reported earlier (182). In the present experiment, sequential nonrandomness of S's² responses occurs regardless of prior training with intensity-random or intensity-ordered stimuli. Blackwell *et al.* (24) describe an elaborate apparatus for automatically controlling stimulus presentation and recording of S's responses in threshold measurement. McLaughlin (136) also describes an apparatus for continuous recording of absolute threshold measurements.

Absolute threshold.—Denton & Pirenne (58) measured absolute thresholds of the human eye to a test field subtending 45° and flash exposures of 5 sec. duration. S observed the stimulus under ordinary binocular vision with natural pupils and without a fixation stimulus. For light of 510 m μ wavelength, intensity at threshold is in the order of 10⁻⁶ erg/sec. per steradian per cm² of the test field. One of the most interesting findings is that the uncertainty range from zero to 100 per cent seeing is reduced to only 0.3 or 0.4 log unit. Thus average sensitivity does not vary from one 5-sec. trial to another by more than ± 0.2 log unit. The authors suggest this increased stability of sensitivity is attributable to the reduced likelihood that quantum fluctuations can influence seeing compared with measurements using small test field and short flashes.

Variation on the problem of area-intensity and time-intensity relationships for absolute threshold is exemplified by two studies measuring threshold for moving stimuli. Basically, the problem reduces to effects on threshold of time of observation (t), velocity (v), and length of the line-shaped area of stimulation (vt). Within the framework of the van der Velden two-quantum theory of the threshold, Bouman & van den Brink (27) investigated all these variables using a 2' target which moved concentrically with the fovea, 7° peripherally. The theory predicts two quanta must be effectively absorbed in the retina within a critical time (t) and area (or in this case, distance) (d) at threshold. When (t) or (d) is greater than the critical values, the total number of quanta at threshold increases proportionally to the probability

of two quanta being effective within these critical area-time units, which means specifically, that the threshold number of quanta should be proportional to $\mu^{1/2}$ and $v\mu^{1/2}$. The results are as predicted, and again two quanta are found to be the critical number. In a somewhat similar but not easily comparable experiment, Pollock (151) measured luminance thresholds with a 1° circular spot of light moving at rates from 50 to 2000 degrees per second across a 20° arc centered at the fovea. As distance was fixed, time of exposure and velocity are confounded. The relation between threshold (log luminance) and log angular velocity is a linear function with a slope slightly less than one.

Riopelle & Chow (159) find that area-intensity relations for threshold are not affected by wide changes in retinal location of the stimulus, at least for test areas subtending less than 1° . This constant efficiency of spatial summation implies that the degree of convergence of rod to bipolar to ganglion cells is unchanged over considerable areas of the retina. The notion is contrary to common assumption, but, as they point out, it is supported by recent anatomical research [Vilter (184)]. Collier (52), in a very careful and well controlled study, finds definite evidence for binocular summation at threshold, significantly greater than would be expected from combination of independent but nonmutually exclusive probabilities for seeing with either eye alone. In fact, the amount of summation is not significantly different from that predicted by the hypothesis that binocular stimulation is functionally equivalent to doubling the area of stimulation in one eye. Collier's seems to be the most definitive of the conflicting studies of this problem.

Brightness.—The effects on brightness of a test stimulus induced by stimulation of another region of the retina poses a significant problem in vision. Several theories have been proposed relevant to the problem, none of which seems adequate at the present time. Three studies directly addressed this problem. Diamond (59) measured the apparent brightness of a small test field by means of a binocular matching technique, as a function of the luminance of a contiguous inducing field, both stimuli lying within the fovea. Test field luminance was varied systematically as a parameter. The fact that practically no contrast occurs when inducing field luminance is less than test field luminance constitutes the new finding. When the inducing field luminance is greater than that of the test field, contrast is directly proportional to the luminance of the inducing field. On the basis of a theoretical argument, Diamond tentatively rejects the hypothesis of Fry & Alpern (75) that contrast is attributable to scattered light acting at the borders of the test field. Leibowitz *et al.* (125), in a very similar experiment, show that contrast decreases as a function of separation of test and inducing fields. When the inducing field is entirely outside the fovea, there is a small residual effect, but it does not seem to be systematically related to inducing field luminance.

Another approach to this general problem of spatial interaction is provided by Alpern (1) who reports a careful and thorough study of the decreased brightness of a test flash produced by a later flash, applied to a near-

by region of the retina. The effect, called metacontrast, increases as an inverse function of the luminance and duration of the test flash (first) and as a direct function of the luminance and duration of the inducing flash (second); and it is maximal when the inducing flash follows the test flash by 75 to 110 msec. As in simultaneous contrast, metacontrast decreases with spatial separation of the two stimuli, but unlike simultaneous contrast, it does not occur when both stimuli fall within a 2° foveal region. Because of the differences of response time between rods and cones, one might think metacontrast is the result of the slow rod effects stimulated by the test flash being inhibited by fast cone effects coming from the inducing flash, but Alpern points to several difficulties of this hypothesis.

A new complication of the problem of brightness constancy is suggested by the findings of Hochberg & Beck (97). They report that apparent orientation with regard to the direction of illumination of a surface influences its perceived brightness, even though the physical orientation and the actual illumination are unchanged in the two conditions of viewing. These results pose difficulties for explanations of brightness constancy in terms of stimulus variables alone, e.g., Wallach's theory that brightness constancy is based on the constancy of ratios of retinal illumination from neighboring surfaces that differ in albedo (187).

Construction of sensory ratio scales from data produced by the procedure of fractionation must assume that a given stimulus yields equivalent sensory magnitudes when presented under different conditions, particularly different states of adaptation. Michels (140) argues, in view of the experimental demonstrations of the effectiveness of "adaptation level" on stimulus ratings, that such an assumption is invalid [see Michels & Helson (139)]. He further demonstrates that Fechner's law, reformulated in terms of "adaptation level," can predict quantitatively the fractionation data used by Hanes (88) to construct the Brill scale.

Brightness influences response time undoubtedly through its relation to discriminability. Bartlett & MacLeod (9), reviving an old and classic problem, investigated reaction time as a function of signal and field luminance. The results provide a more systematic account than heretofore available of effects over a wide stimulus range under conditions controlling area, duration, retinal locus, contrast, etc. The results are described by hyperbolic functions,

$$T = \frac{1}{B(\log I/I_0)} + K,$$

in which B is the slope constant, I is flash luminance, I_0 is the limiting flash intensity at which RT^∞ approaches infinity, and K is the limiting latency which RT approaches as I becomes very large. Gerathewohl (79) finds the conspicuity of a flashing signal, as measured in terms of response time, to be superior to that of a steady signal only under conditions of low contrast.

Adaptation.—An investigation, using parallel psychophysical and ERG² procedures, of the onset of light adaptation in rods was reported by Boynton

& Trieman (30). Sensitivity was measured in terms of threshold for test flashes delivered before, simultaneously, and after the onset of the adapting light. The results parallel those of Crawford (55) and Baker (7) for photopic levels, i.e., sensitivity starts decreasing even before the onset of the adapting light, drops precipitously to a minimum about 0.2 sec. after the onset of the adapting light, and then recovers slightly, fast at first and then more slowly. ERG records show that the anticipatory effect is a result of the longer latency of responses evoked by the test flash as compared with those to the adapting light. The response processes (b-waves) overlap in time even though there is a temporal interval between their respective stimuli. Their results provide additional argument for hypothesizing neural factors in adaptation, an hypothesis supported, on quite different grounds, by Bouman & ten Doesschate (28). These authors reason from the van der Velden hypothesis (see above) that if a changed concentration of visual purple is the only cause of decreased sensitivity in light adapted states, the proportionality between area of the stimulus and number of quanta at threshold for areas larger than the critical (Ricco's) region should not differ from the proportionality observed for the dark adapted eye. Their result for the light adapted eye shows an increased slope in the function relating energy at threshold (i.e., number of quanta) and area of the test stimuli, which they believe implies a change in the area-intensity relationship, presumably based on neural factors.

Using a similar psychophysical procedure to that of Boynton & Trieman (see above), Baker (8) investigated the early onset of dark adaptation by measuring thresholds of test flashes presented before (incremental threshold), simultaneously (simultaneous threshold), and after (absolute threshold) the cessation of the adapting stimulus, both in the fovea and periphery, and over a range of adapting luminances. Clear evidence is found for an anticipatory rise in threshold prior to off-set of the adapting light (see above). The simultaneous threshold is highest, then as the test flash follows later and later after the off-set of the adapting light, thresholds drop very rapidly for about 0.3 sec. and then more slowly as the curve becomes that of the usual course of dark adaptation. The anticipatory rise and early, rapid drop in threshold is more marked for foveal stimulation and high adapting luminance than it is parafoveally and with lower adapting luminances. The abrupt rise and fall of threshold associated with the cessation of the adapting light suggest (as with light adaptation) that neural factors play a part, at least in the early stages of dark adaptation.

Not only does a stimulus in a nearby region of the retina decrease brightness of a test stimulus (contrast), but it also raises the threshold in the test region. Does the increased threshold simply result from light adaptation of the test region by stray light or does it reflect some other mechanism? Boynton *et al.* (29) attacked this problem by comparing the time course of changes in sensitivity induced by light adapting effects of a direct, veiling stimulus superimposed on the test area with the effects of an indirect, glare stimulus located some distance away, on the retina. For both conditions

thresholds of a foveal test stimulus were determined before, after, and simultaneously with the onset of the adapting stimuli. The conditions of both direct and indirect adapting stimulation yield typical light adaptation curves of identical time course, a finding which leads the authors to conclude that the action of the distant stimulus is attributable to adapting effects of stray light and not to trans-retinal action.

Brown & Diamond (35) described a device for simulating the visual display of PPI scopes, and in a subsequent study Brown *et al.* (36) used the device to investigate effects of duration of light adaptation and the ratio of target to background luminance on adaptation time to the test stimulus level required for detection of the targets. As one might expect, target-detection time increases with duration of light adaptation up to the time required for the eye to reach a steady state, and it decreases with increased target-background contrast.

A somewhat perplexing state of affairs appears to have been cleared up. Some earlier studies were interpreted as indicating dark adaptation proceeds more rapidly in red light than in total darkness. A very thorough study by Miles (142) demonstrates that use of red goggles does not result in as complete dark adaptation as does a comparable period in total darkness. Using different methods, Smith & Dimmick (174) find no consistent differences in degrees of dark adaptation yielded by red goggles and total darkness.

Flicker.—Landis (117) has performed a useful service to research workers interested in cff by compiling a very complete annotated bibliography on flicker and related topics covering the period from 1790 to 1952. The 1300 items are listed alphabetically by author. Crozier and his associates (mostly E. Wolf and G. Zarrahn-Wolf) published some 50 papers between 1933 and 1944, most of them on cff. However, there has never appeared an integrated summary of this important work until Landis (118) recently summarized the essential empirical and theoretical contributions of these workers. Exception will undoubtedly be taken to some of the critical comments at the end of the paper. Landis (119) has also written an excellent review of the experimental work relating some 20 classes of variables to cff.

A number of studies contributing to our basic understanding of cff have appeared recently. Battersby & Jaffe (12) investigated cff as a function of light-dark ratio and duration of exposure to the intermittent light at a constant flash luminance (10.9 C/ft^2). Multiplying F/sec. at fusion by duration of exposure yields the number of successive flashes for cff, which plotted as a function of exposure time, shows that the number of flashes per unit time is constant at cff. The slopes are very similar for the three light-time fractions used (20, 50, and 80 per cent). If the critical number of flashes is plotted as a function of total light energy in the exposure period, the curves for different light-time fractions differ greatly in slope, and the conclusion is drawn that the smaller the light-period in the cycle is, the greater the number of flashes per unit energy required for cff. Experimental findings regarding central and peripheral regions of the retina and cff are confusing and contradictory. Al-

perrin & Spencer (2) attribute this state of affairs to inadequate control of pupil size in many previous experiments. When effective pupil size is fixed cff drops rapidly from 0° to 3° and more gradually from there to 15° as the stimulus is moved from the fovea to the periphery. When the natural pupil is the limiting stop in the system, cff does not decrease as the target moves peripherally, but instead, increases slightly. Experimental variation of effective pupil size influences cff in the manner to be expected from the corresponding variation in retinal illuminance. Berger (17) reports that a steadily illuminated surround does not influence cff of a centrally fixated target when the latter is quite small or very large. Maximal effect of surround on cff occurs with a test area of $1'$ subtense in diameter. Cff rises as surround illumination increases, reaching a maximum when the test area and surround are of equal luminance. With more intense surrounds, cff decreases. On the other hand, a veiling steady light superimposed on the flicker test area, diminishes cff proportionally to its luminance. Because veiling light influences cff in a different manner from that of a nonveiling surround, Berger believes the effects of surround are attributable to interaction in the retina (contrast mechanism) and not to veiling effects of stray light. In another experiment Berger (16) investigated the relation of cff to area of test patch in the fovea ($27'$ to 4.6° field diameter) at three levels of flash intensity, with and without illuminated surround. Plots of log area and cff show an S-shaped curvilinear relationship. Increased flash intensity merely displaces the curve upwards on the cff coordinate. However, variation in surround luminance changes the shape of the cff-area relationship.

Perrin (150) reported in full the experiment he conducted almost 20 years ago on binocular flicker. As several recent studies have shown also, interocular differences in phase of flashes to the eyes influence cff. Perrin proposes that we call it the "Sherrington effect." Most significant is the finding that the increment in cff when flashes are synchronous in the two eyes, as compared with cff when they are alternate, is a constant proportion of mean cff over a wide range of flash luminance. Unpublished data of the reviewer's (178), however, indicate that the Sherrington effect is largely independent of mean cff and flash luminance. The source of this difference in results of the two studies remains to be determined.

Studies using cff as a measure of individual differences in relation to various physiological states continue to appear frequently. Landis & Hamwi (120) measured cff hourly over a 10-day period in conjunction with measures of body temperature and basal metabolic rate. A more or less constant pattern of diurnal variation, unique to each individual, was found, which in some Ss was related to fluctuations in body temperature. BMR of the Ss were all within normal range and were related more to variability than to level of cff. Grandjean *et al.* (82) find variations in cff with time of day, distribution of rest periods, and work output which lead them to conclude that cff might be a useful index of fatigue. However, Ryan *et al.* (164) did not find a drop in cff after prolonged reading in all cases. They suggest that cff

is perhaps sensitive only to more extreme degrees of fatigue or to general stress. Irvine (106) determined in a sample of patients that cff averaged about four F/sec. less in paretics than in schizophrenics which he interprets as reflecting the known organic lesions in the former disease. Rokseth & Lorentzen (161) report that blood alcohol concentrations of .03 to .04 per cent combined with hypoxia induced by simulated altitude of 10,000 ft. results in a greater decrease in cff than either alcohol or hypoxia separately.

Electrophysiological studies of cff will undoubtedly contribute increasingly to our understanding of the mechanism underlying flicker discrimination. Enroth (66) presented results of a thorough investigation using micro-electrode-recorded responses of single retinal elements in the dark-adapted cat's eye. She finds for both on- and off-discharges, that cff (indexed by disappearance of spike bursts synchronized with the flashes) is related to the initial rate of impulse discharge which she considers, assuming Adrian's concept of the dependence of sensory intensity on impulse frequency, to explain the relationship between flash intensity and cff. In a later study she (67) showed this linear relationship between fusion frequency and initial spike frequency to hold true independently of state of adaptation, flash intensity, and wave length. Dodt & Wirth (62) find that flash intensity at which a rod-cone break occurs in the flicker curve, as measured by ERG, is related to the relative proportions of the two types of receptors in retinas of different species. The break occurs at 1 to 15 lux in man, 200 to 500 lux in cat, and around 5000 lux in guinea pig. (1 lux = .0929 ft. C, a unit of illuminance.) The pigeon yields a simplex curve, steeply rising to a maximum of about 140 F/sec. As man and pigeon have quite similar numbers of optic nerve fibers, the authors conclude that receptive fields must be smaller in pigeon to account for its high sensitivity to flicker. Dodt & Wadenstein (63) find the cff-flash intensity contour in man, as determined by an ERG index to be very similar to the curves obtained by the usual psychophysical methods.

Acuity.—Ludvigh (133) measured the accuracy with which Ss can line up a center dot with two outer points as a function of their separation. At 15' separation of the reference dots, resolution reached a minimum of 2" of arc. A misalignment of one-tenth of a cone width can be perceived when the reference images are only 120 cone widths apart. Ludvigh believes the dependent relation between accuracy of this "direction sense of the eye" and separation of the reference points cannot be explained on the basis of an average local sign derived from summed excitation of a large number of cones because, presumably, variation in separation of reference dots involves no change in the number of cones participating. Leibowitz (124) varied retinal illuminance and pupil size in an attempt to determine the basis for the puzzling fact that acuity resolution is better for vertically or horizontally oriented gratings than it is for gratings in oblique orientation. He believes he can reject dioptric factors as the source of the phenomenon because the effect still occurs under conditions of high retinal illuminance and large pupil

diameter. Under these conditions small changes in image contrast that might be induced by dioptric factors, presumably, do not affect acuity. Brown (37) reports additional work on the course of dark adaptation as indexed by an acuity end point. Assessment of the effects of various levels of preadapting luminance constitutes the novel aspect of the study. The finer the resolution required, the higher the dark adaptation curves lie on the log threshold ordinate, and increasing levels of preadaptation causes higher initial thresholds and more gradual decline of the dark adaptation curve. Miles (143) also showed that thresholds for form discrimination are considerably higher than light detection thresholds for both rod and cone levels.

Gerathewohl & Taylor (81) investigated the possibility of using the Brücke-Bartley effect to improve readability under conditions of low contrast, but found instead, that slow flicker (9 and 15 F/sec.) resulted in decreased readability as compared with that under steady illumination. They also varied light-time fraction of the flicker cycle, and from their results, concluded that Talbot brightness determines print visibility. However, Senders (167) replotted their data in terms of log luminous reflectance at threshold readability as a function of log percentage light-time and showed that the curves fall considerably short of the effect predictable from Talbot's law. In fact, their data are very similar to her own earlier results (166) showing that regardless of flash frequency, less energy is required for resolution when the light is intermittent than when it is steady. Beck (14) proposes that the Stiles-Crawford effect (differential directional sensitivity of the foveal cones) has a functional significance for vision in counteracting the degradation of the image by aberrations and scattering of light within the eye.

Campbell (44) followed changes in accommodation of the human lens as a function of luminance and size of test object by photographing the Purkinje-Sanson image formed by the anterior surface of the lens. Results definitely indicate foveal cones are the receptors for the accommodation reflex. And Campbell & Primrose (45) showed that the human lens in darkness has about 0.8 diopter of accommodation which is insufficient to account for the full degree of night myopia which generally amounts to about 1.5 diopters. Bartley (11) investigated the effects of variation of luminance and distance of an annular surround of constant angular subtense on acuity resolution of a target, constant in size and distance from S. A dim surround has little effect, but a bright surround tends to determine accommodation distance, thus showing that the eyes do not always accommodate for the distance of the object of attention.

Theories postulating a significant role of physiological nystagmus in acuity [e.g., Marshall & Talbot (137)] have stimulated considerable work in recent years on these tiny eye movements occurring during fixation. Evidence for rapid adaptation of contour vision when the image is steady with respect to given receptors has been provided by Riggs *et al.* (157). These investigators devised a compensating optical system which caused an acuity target

reflected from a mirror on a contact lens, to remain fixed, relative to the retina, regardless of eye movements. The appearance of the target under three conditions of viewing was investigated: (a) no movement of the image on the retina, (b) normal fixation, and (c) exaggerated movement of the image, i.e., eye movements yielded double the normal image movement. In the condition of no movement of the image, a fine hairline fades out after observation of only a few seconds. Larger contours take longer to fade. With normal fixation, fine lines often fade out sporadically, but thicker lines seldom disappear. In the exaggerated condition, even fine lines seldom disappear, and because the ocular-motor system is provided with double the normal feedback of cues indicating drift of fixation, the image seems locked in place and fixation is effortless. The results indicate definitely that small involuntary eye movements during fixation prevent rapid washing out of differential response to a brightness contour and consequent disappearance of the object. With very short exposures (less than 0.1 sec.), vision was best under conditions of no motion of the image as compared with vision under the normal and exaggerated conditions of image motion. A related experiment was described by ten Doesschate (64) who, by an entirely different method, formed a fixed image on the retina. Occasionally, a forced pendular nystagmus with low frequency and large amplitude occurred, especially after S voluntarily turned his eyes to one side. The author suggests the phenomenon is caused by loss of visual cues for corrective fixation movements. Because of the gross sensitivity of the proprioceptive system of eye muscles, the movements are large. However, he thinks the nystagmus also depends on emergence of a more primitive mechanism underlying the fixation reflex mechanism. Riggs *et al.* (157) did not report such a phenomenon with their method.

By directly photographing blood vessels on the cornea, Higgins & Stultz (93) were able to measure frequency and amplitude of involuntary eye movements during monocular and binocular fixation. Average frequency of the fine tremors of the eye is 50 c.p.s. with an average amplitude of $1.2'$ of arc. Riggs *et al.* (158) report that during an exposure of 0.01 sec., the eye is virtually stationary. Exposure of 0.1 sec. yields average displacements of $25'$ subtense (about one cone width), and with 1.0 sec. exposure, average excursions are about $3'$ of arc. They measured the movements by recording displacement of a beam of light reflected from a mirror on a contact lens.

Craig & Lichtenstein (54) measured the frequency of partial or total disappearance of a line, fixated at one end, as a function of angular orientation of the line. Frequency of disappearance is greatest when the line is oriented obliquely and least when it is oriented horizontally or vertically. The authors suggest that small nystagmic movements may be greater in the horizontal and vertical direction, thus slowing the adaptation process underlying the disappearance more in those orientations than in oblique orientations.

Color vision.—The problem of color vision continues to stimulate rich research productivity, bewildering in its multiplicity and diversity of special

problems, theories, and techniques. Effects of color adaptation on the hue, brightness, and saturation of other colors is a currently active research area. Jameson & Hurvich have published their excellent studies, reviewed in part by Riesen (156) from preliminary reports, on the effect of white and chromatic adaptation on foveal spectral sensitivity (104, 108). Their results support the notion of a specific white mechanism responsible for brightness with subsidiary contributions to luminosity from chromatic mechanisms. In a third paper (105) they investigated effects of chromatic adaptation on supra-threshold, heterochromatic brightness matches. Again, foveal luminosity was shown to be influenced by chromatic adaptation in a manner very similar to the effects demonstrated in terms of luminosity thresholds and reported in the first two papers. "... adaptation to a given hue causes a relative decrement in the luminosity of the spectral stimuli which evoke that hue" (p. 222).

Brindley (34) investigated the effect on various color matches of prior adaptation to very bright chromatic stimuli. Some very marked effects are reported, such as temporary dichromacies and even monochromacies induced in normal eyes. Hunt (103) repeated his previous study (102) on the effect of luminance of an adapting light on the appearance of colors, using 1° color fields instead of the 20° fields used previously. The results are very similar to those of the previous study, i.e., only small changes in hue occur, but marked increases in saturation with light adaptation were noted. In a theoretical paper, Bridgman (33) argues that the mesopic luminosity curves represent the progressive shift from one receptor system to the other as the sensitivity of one becomes greater than the other at different wavelengths as intensity level is changed.

Bachem (6) reports that the color of short wavelengths reverses itself from violet back toward indigo-blue (as previously described by Helmholtz) and then fades out with decreasing saturation as wavelength is progressively shortened. Bachem believes the fundamental color of the short wave end of the spectrum is indigo-blue, contaminated to violet in the middle of the region by a secondary maximum of the hypothetical red receptor. Spectrographic study of fluorescence of eye media leads Bachem to reject Helmholtz's hypothesis that intraocular fluorescence causes the "lavendar-gray" color of ultraviolet light. Burnham & Newhall (43) report that varying shape and orientation of the test patch has no significant effects on perceived colors as determined by binocular matching, but a 20-fold area increase in a test patch of 25 min.² causes significant increments of saturation. Plots on chromaticity diagrams show that decreased target size causes the colors to converge toward the blue corner in a way resembling the hypothetical confusion lines of tritanopia. Burnham (42) describes an interesting phenomenon which appears to involve mixture of contiguous colors under conditions which ordinarily would be expected to yield contrast, e.g., a red appears lighter than in normal viewing when a white pattern runs through it and darker when a black pattern overlays it.

Stimulated in part perhaps by problems of color television, MacAdam (135) presents a mathematical-theoretical discussion of the dependence of color mixture functions on the choice of primaries and develops formulas for deriving all orthogonal sets of color mixture functions from a given set of primaries, a procedure facilitating transformation of any one set of mixture functions to another. In a related methodological paper, Bingley (19) develops a procedure for converting points on one chromaticity diagram to corresponding points on another. The procedure would be especially useful in converting arbitrary chromaticity diagrams (e.g., one based on the given primaries of a colorimeter) to the standard C. I. E.² diagram.

Because of the considerable individual differences he finds in subjective hues seen on a Benham disk, Knehr (113) favors a central locus for the phenomenon. Contrariwise, one may recall Hess' report (91) of consistent perception of subjective hues produced with a modified Gehrcke pattern. His (92) negative findings, when he combined components of the pattern binocularly, strongly favor a peripheral origin of subjective colors. Miles (141) has reported a careful study and description of the entopic phenomenon called "Maxwell's spot." He agrees with Walls & Mathews (189) in thinking that the central ring of the spot reflects receptor distribution patterns and not effects of macular pigmentation. An interesting paper of de Vries *et al.* (185) reports no evidence for polarization of light in the sense cells of mammalian eye, but their results do support Helmholtz's hypothesis that the phenomenon of Haidinger brushes results from polarizing properties of the yellow macular pigment.

In recent years there have been an increasing number of careful laboratory studies of visual characteristics of color-deficient eyes. Several such studies appeared this year. Thomson & Wright (179), with better methods than Wright (194) had used earlier, determined confusion loci of tritanopes in matching wavelengths in the yellow-green with those in violet. The extrapolated confusion lines clearly converge to a co-punctal lying in the blue corner of the chromaticity diagram, outside the spectral locus, indicating, in the authors' opinion, strong evidence that tritanopia represents a reduced form of trichromatic vision. By advertising in the Sunday newspaper, Weale (191, 192) was able to locate five and measure the visual characteristics of three cone monochromats (a type of defect characterized by total loss of color vision but normal acuity). He concludes in part that the deficiencies in this type of color blindness must be located in postreceptive structures. Support for this point of view is provided by Fincham (69) who measured with the coincidence optometer, the effectiveness of monochromatic light (yellow) to elicit accommodative changes as compared with the effectiveness of an equivalent yellow composed of a mixture of red and green wavelengths, in normal, dichromatic, and monochromatic Ss. An earlier study (68) had shown that color fringes from chromatic aberration can be an effective stimulus to the accommodation reflex. Results of the present study, Fincham believes, show R-G blindness to be retinal in origin, but part of the defect

of cone monochromats originates centrally to the branching of the reflex pathways to the midbrain. Sloan (170) administered a number of visual tests to 19 cases of achromatopsia (rod-monochromatism, i.e., besides loss of color vision these persons have photophobia, and often nystagmus, and suffer loss of acuity). Her most interesting findings are from dark-adaptation measures which indicate that even the central fovea of these Ss is duplex in function, having both scotopic and photopic receptors. Hardy, Rand & Rittler (89) described the new H-R-R Polychromatic Plates and reported a study of the validity and reliability of these plates as a test of color defect. Results indicate these plates constitute a remarkably effective test.

Space does not allow a thorough coverage of physiological and comparative studies of color vision, but several papers which seemed especially interesting are reviewed. Walls (190) has written a characteristically critical and scholarly (if sometimes contentious) summary-review of the comparative studies of the lateral genicular nucleus (LGN). He criticizes severely Clark's (51) hypothesis that the laminar structure of the LGN represents anatomical pathways of the three color receptors postulated by trichromatic theory. However, Walls also contributes his own interpretation of the functional significance of these laminar structures. To oversimplify, he identifies the small cell laminae with cone pathways and the large celled laminae with rod pathways, the latter being different in nasal and temporal retina. His interpretation, he insists, explains Clark's findings that degeneration in the LGN of monkeys occurs after about one month exposure to red light. However, Walls may be premature. Chow (50) has recently repeated and elaborated the experiment, using even longer exposure (eight months) to colored light. The results are negative, both as regards behavioral tests of color sensitivity and histological changes in LGN. Walls' theory also requires functional independence of the laminae, but since he wrote, Bishop & Davis (20) have reported evidence for neuronal interaction at the level of LGN in the cat.

Photopic luminosity curves determined by Armington (4) employing an ERG index, agree well with the I. C. I.² curves except for a markedly heightened sensitivity in the blue end of the spectrum. The excess sensitivity in the blue was less in Ss suffering night blindness, leading Armington to conclude that stray light in the short wavelengths was stimulating scotopic receptors outside the primary area of stimulation. ERG responses to a smooth shift in wavelength which could not be cancelled out by intensity adjustments are reported by Burleigh & Forbes (41) to differ, depending on the direction of the shift. They interpreted the effect as a result of differential involvement of rods and cones, depending on the direction of the shift. However, a similar though smaller effect was observed in what they presumed to be the all-cone retina of the turtle. There now exists histological evidence of rod-like receptor cells in turtle retinæ, and Armington (5) has found, both in terms of ERG and behavioral methods, presumptive evidence that turtles (*Pseudemys*) possess functioning photopic and scotopic mechanisms.

Continuing his work on the vision of the cat as measured by behavioral methods, Gunter (86) now reports their photopic sensitivity curves. Comparison of the photopic curve with scotopic curves previously reported (85) clearly indicates existence of a well-defined Purkinje shift. And comparison with Granit's (83) electrophysiological measures of the cat photopic curves shows a discrepancy in the blue region of the spectrum, as compared with the behavioral curves, quite similar to the discrepancy reported for the human eye by Armington (4) (see above). The cat's photopic curve contains two well defined maxima, at 470 $m\mu$ and 555 $m\mu$, one on either side of the scotopic maximum. Using four animals from the above experiment and four naive animals, Gunter (87) tested the cat's ability to discriminate wave length. The results are convincingly negative, a finding, Gunter points out, in agreement with all previous attempts to demonstrate color vision in cat except one by Kalischer (111) who trained his animals with dyed and undyed food. Other experiments have reported color vision in cats but usually luminosity cues were not adequately controlled. Still the cat has a duplex retina and duplex visual functions, e.g., Purkinje shift in luminosity and rod-cone break in the flicker curve (62), as well as receptors differentially sensitive to wavelength (Granit's modulators), which, of course, does not mean that they necessarily possess color vision. In view of Jarvik's (109) demonstration of rapid acquisition of color discrimination in primates with Kalischer's colored food technique, perhaps one last, well controlled try on cats should be made, taking advantage of that method, but likelihood of demonstrating color discrimination in cats seems very small. Meyer *et al.* (138) also report inability of cats to discriminate color.

Wald *et al.* (186) have synthesized a new photochemical pigment which is blue sensitive and which the investigators named "cyanopsin." Rhodopsin, iodopsin, and cyanopsin, the authors point out, have maxima of absorption spaced about 60 $m\mu$ apart in the visible spectrum, well suited to provide the basis of a system of trichromatic vision, although, it must be pointed out cyanopsin has not yet been found in a living retina.

PERCEPTION OF FORM AND MOVEMENT

Form.—The ability to discriminate differences between two patterns of dots, in terms of relative displacement of one dot as a function of number and average separation of dots, was investigated by French (72). Patterns were presented successively and S responded "same" or "different" to each pair. Ss made faster and more accurate judgments in detecting differences between three-dot patterns than any other dot-number patterns. Also detectability of change in pattern decreases as the average distance between dots in the patterns is increased. French hypothesizes that Ss respond to the proportion of relations modified by displacement of one dot rather than to the absolute number of such relations. Thus, displacement of one dot in a three-dot pattern changes two-thirds of the possible interdot distance relations; similarly, in a four-dot pattern, one half the interdot distance relations are changed,

and so on. On the other hand, identification of a dot pattern from memory is optimal for a degree of complexity represented by patterns of six to eight dots (73). The ability to recognize a given pattern of dots as a function of the masking effect of adding extra dots improves as the ratio of number of target dots to "noise" dots increases up to target-to-noise ratio of about 3:1 (74).

Hochberg & McAlister (96) had Ss indicate at random intervals whether various Kopfermann cubes appeared in three dimensions or as a two dimensional pattern. The percentage observations describing the patterns as two dimensional varied inversely with the number of line segments, number of angles, and number of points of intersection in each cube pattern. The hypothesis is suggested that "... probability of a given perceptual response to a stimulus is an inverse function of the amount of information required to define that pattern ..." (p. 364).

Figural after effects.—The phenomenon of figural after effects and "satiation theory" have stimulated some theoretical thoughts and a minor controversy. Luchins & Luchins (129, 130) in two historical, systematic papers develop what they consider to be the implications of the Köhler-Wallach hypothesis (115) for the Gestalt principles of perception. George (77, 78) finds eye dominance has no relation to figural after effects and also defends the Osgood-Hyer (148) interpretation of figural after effects against criticisms leveled against it by Smith (172). Osgood (149) also replied to Smith. The main bone of contention concerns the existence of an "attraction" effect, i.e., a shift of the T-figure toward the locus of the I-figure. Generally one finds that the test figure or some part of it tends to shift away from the area previously occupied by the inspection figure. Smith interprets Köhler & Wallach [(115), see p. 292] as having described such a phenomenon. Osgood (149) thinks Smith's is a misinterpretation. In a more recent paper Smith (173) presents three diagrams which he asserts will demonstrate the phenomenon of "attraction."

The usual method of measuring figural after effects consists of comparison by S of two test figures, one of which is exposed in a region of the visual field previously satiated by prolonged exposure to the inspection figure, making the assumption that without the previous satiation the two test objects would appear identical and symmetrical. Others have criticized this assumption, but Brown (39) points out that his findings of marked differences in apparent size on opposite halves of a visual meridian definitely invalidates the assumption and represents a factor that must be controlled. He also suggests that changes in the Müller-Lyer illusion with repeated testing might be related to the gradual variation over time that he observed in the half-meridional disparities and not necessarily attributable entirely to satiation as interpreted by Köhler & Fishback (116) [see Brown (38)]. Carlson (46) satiated one or the other of two alternatives of ambiguous test figures by fixation of similar inspection figures, modified slightly to prevent reversal. Results with the reversible perspective figure support predictions of Köhler's satiation hypothesis in that the first response to the reversible figure tends

reliably to be in the opposite orientation from the inspection figure and when reversal does occur, the unsatiated orientation predominates. On the other hand, first perception after inspection of one of the alternative forms of the "wife-and-mother-in-law" figure tends to be the same as the inspection alternative. Inspection satiates a spatial but not a "meaning" location according to Carlson. Culbert (56) had Ss inspect grid patterns differing only in the amount of deviation from horizontal of their cross lines. Afterwards they judged the tilt of a test line relative to perceived horizontal. He finds that Ss' phenomenal horizontal is shifted toward the coordinates of the distorted inspection field. Although the author does not discuss the possibility, it seems likely that the results represent an instance of the more general Köhler-Wallach principle, i.e., the test line tends to be displaced away from the locus occupied by the cross lines in the inspection figures.

Figural after effects in relation to real and apparent movement was explored by several investigators during the current year. Livson (126) finds that inspection of apparent movement decreases the subsequent estimated extent of autokinetic movement. Inspection of the same flashing lights timed so they do not produce apparent movement, has no effect on the extent of autokinetic movement. Livson favors an interpretation in terms of isomorphism. Brenner (31) takes exception to the conclusion of Deatherage & Bitterman (57), who found that a state of satiation affected the threshold and path of seen movement and interpreted their findings as supporting both Wertheimer's "short circuit" theory of apparent movement and the Köhler-Wallach theory of figural after effects. She found that all of the following, a 2 min. inspection period of visual stimuli, auditory stimulation, voluntary activity (pacing up and down), and solving mental arithmetic problems, were effective in shifting the before and after limiting thresholds of apparent movement. Because all of these diverse conditions influence apparent movement thresholds, she thinks more general conditions than "short circuits" and satiation in area 17 are involved. Shapiro (168) also finds that inspection of a figure in the same region of the visual field in which apparent movement stimuli are presented changes the threshold for apparent movement. Grindley & Wilkinson (84) investigated after effects induced by inspection of a rotating spiral pattern. Nineteen of 20 Ss, all of whom easily saw the well-known effect of apparent outward expansion when the spiral was suddenly stopped, reported that, when an entirely uniform white surface was viewed after observation of the spiral, this surface appeared to expand and approach. In addition the surface appeared to acquire vague structure which was, however, never reported as spiral shaped.

Apparent and real movement.—The most interesting and novel research findings on the problem of perceived movement are those of Motokawa and his co-workers. The experiments are based on his technique of determining thresholds of electrically stimulated phosphenes following various sequences of photic stimulation [see Gebhard's review (76)]. In the present experiment (145) a spot of yellow light was moved across the visual field at various veloc-

ities. At different points in the path of movement a brief flash (2 sec.) of white light was presented and then phosphene thresholds in terms of stimulus voltage were determined. Plotting the percentage increment of phosphene threshold over its resting level as a function of distance from the start along the path of the moving stimulus yields, what Motokawa calls, a "trace curve." In general, trace curves increase in height with distance, showing a rising gradient in the direction of movement. The provocative findings are that characteristics of the trace curve correlate with various perceptual characteristics of moving objects. In a second paper Motokawa & Ebe (146) explored the path of apparent movement with the phosphene-threshold technique. They find that the region between the primary stimuli is equivalent in its effects on phosphene threshold to that of the actually stimulated region when the timing and intensity of the primary flashes are such as to yield optimal seen movement, i.e., the two processes aroused by the separate stimuli are fused under conditions of optimal movement. The authors, however, wisely do not propose that retinal events constitute the exclusive substrate underlying apparent movement.

Carter (47) investigated the use of phi movement as a measure of cerebral dominance by a technique originated by Jasper (110). The technique consists of locating the two stimuli at different distances from S who fixates one of them and reports which of the diplopic images of the other stimulus appears to be involved in the apparent movement. Carter's results with a modified Jasper procedure and with a similar test of his own design (S reports direction of movement involving three equidistant stimuli) lead him to conclude that cerebral dominance determines the response but that ocular dominance does not influence direction of the apparent movement. In a paper, primarily theoretical in content, Saucer (165) proposes a formulation of isomorphism based on the Hebb concept of matrices of cell assemblies and a hypothetical construct of his own, the "cortical projection plane," which is postulated to be isomorphic to real space. His theoretical formulation and experimental results (only sketchily described) lead him to the conclusion that underlying beta movement is a "sensory-sensory transfer of an entire organized process" (p. 558), a conclusion quite similar, but based on different postulated mechanisms, to Wertheimer's original "short circuit" hypothesis.

Conklin *et al.* (53) described an apparatus for measuring thresholds of visual discrimination of direction of movement, and in a later paper Brown & Conklin (40) report measures of the lower threshold of visible movement as a function of exposure time. Thresholds decrease rapidly at first and then more gradually as exposure time increases to about 9 sec. The stimuli appeared on an invisible background in the present experiment. Comparison with earlier measures with visible background [Dimmick & Karl (60)] indicate that the limen for perceived movement without visible background is about seven times greater at 0.5 sec. exposure and 17 times greater at 4 sec. exposure than it is when the stimuli are seen on a visible background. Sweet

(177) measured latency differences for different retinal locations in one eye in terms of sensitivity to discrimination of temporal differences between two flashes, as a function of spatial separation of the flashes, and state of adaptation. The criteria for judgment were (a) temporal order, i.e., which flash came first, (b) flicker, and (c) apparent movement. The last two criteria were necessary because in the periphery, the adjacent lights were often indistinguishable. The results indicate that latency is 5 to 25 msec. greater in the periphery than in the fovea, with considerable individual variation. Just noticeable lag between two adjacent flashes in the fovea is about 5 msec. The criterion of apparent movement yields precise discrimination in the periphery, even when the two lights are spatially indistinguishable.

Rohrer & Hoffman (160) also described an apparatus designed to provide accurate movement of a light stimulus. Hoffman *et al.* (98) find that training sessions in which Ss estimate the extent of real movement of a point of light, exposed within a reference frame provided by a dimly visible double ring, influence estimates of the extent of autokinetic movement in later sessions. Luchins (131) finds that as luminance of a surround is gradually increased, autokinetic movement of a tiny central dot of light seems to move less often, through shorter distances and with longer latencies. The level of background luminance at which movement ceases seems to vary widely from person to person. In another paper (132) he reports that autokinetic movements cover greater distances at more rapid rates when the light is viewed peripherally out of the corner of the eye. Bartley & Wilkinson (10) find that higher luminances are required to see gamma movement in large targets than in small ones, and with rapid rather than slow onset of the flash. Low luminance yields no gamma movement, and stray light interferes with seeing the movement.

PERCEPTION OF SPACE

Stereopsis and other binocular interrelations.—Stereoscopic perception of depth is usually considered to be dependent on binocular fusion of slightly disparate images in the two eyes. However, as Ogle (147) points out, it has been known for a long time that an impression of depth may result when the two images are seen doubled, but few systematic studies of the phenomenon have been made. Ogle measured the precision and validity of stereoscopic depth perception as a function of degrees of retinal disparity sufficient to yield double images, of both test and reference objects. Precision of depth settings decreases exponentially as disparity increases. Perception of depth is, however, remarkably valid out to a limiting degree of disparity. Sloan & Altman (169) find, when all monocular space cues were eliminated, that artificial aniseikonia produces constant errors on the Howard-Dolman test approximating those expected on the basis of theory. However, the authors believe that such moving rod devices are not suitable screening tests for aniseikonia because of the rather large individual variability relative to the normal range of errors that are observed. Lothridge (128) investigated the effects on stereoscopic settings of vertical disparity and declination of target

slits. Vertical disparity had no effects on mean settings but tended to decrease precision of the settings. The effects on precision attributable to vertical disparity are not marked until the disparity exceeds a tolerance range about equal to Panum's fusional areas. In a mathematico-theoretical paper, Blank (25) discusses the Luneberg (134) theory of binocular space, bringing some new experimental data within the framework of the theory which adds support to the hypothesis of the hyperbolic character of visual space. The relation proposed by Luneburg between visual and physical coordinates of space is found by Blank to be only a special case of a more general transformation.

Fischer & Haberich (71) and Fischer (70) argue that the difference in latency between the images in the two eyes is the chief factor in the Pulfrich phenomenon, but also significant are optokinetic-like reactions evoked by the swinging pendulum which cause changes in convergence. Dittler *et al.* (61) and Rosemann & Buchmann (162, 163) take exception on theoretical and empirical grounds. In one experiment they recorded eye movements electrically while S fixated the pendulum and while fixating a stationary point. In the latter case no oscillations of the eyes, synchronous with the pendulum, were recorded, but nonetheless the characteristic Pulfrich effect was observed. A more definitive experiment on this problem is reported by Arden & Weale (3). They measured the time interval between flashes required for apparent simultaneity of two lights, one viewed by each eye, as a function of intensity ratio between the two lights. Because of the differential dependence of latency on intensity in the fovea and periphery, the authors suggest the Pulfrich effect depends largely on rods, functioning at photopic levels. To test their hypothesis they attached a light bulb to a pendulum and observed its filament through a deep red filter which restricted stimulation almost entirely to cones. In addition, a neutral filter was placed before one eye. No Pulfrich effect was seen. Viewed through a blue filter in similar manner, allowing stimulation of rods, a marked Pulfrich effect appeared. Bouman (26) finds that simultaneous and successive incremental thresholds, measured in one eye, are unaffected by various conditions of stimulation in the opposite eye, even if the stimuli are on corresponding points. So long as area or time differences or disparity prevents fusion, the threshold of one eye seems independent of stimulation of the other. However, if characteristics of the stimuli in the two eyes are such that binocular fusion can occur, the component stimuli in the separate eyes become indistinguishable, and it is impossible to measure the thresholds in one eye.

Size, distance, and constancy.—That different regions of the retina yield variation in apparent size of identical stimuli has been known for many years. Brown (38) recently reworked this old problem and uncovered new and interesting findings. He had Ss equate the length of two luminous lines lying on opposite halves of a visual meridian. Results showed very consistent differences in apparent size between opposite halves of the same visual meridian which he called "half-meridional disparities" (HMDs). Repeated meas-

urements of HMDs with right and left uniocular regard and with binocular regard over a period of two to nine weeks showed differences between HMDs of right and left eyes in both vertical and horizontal meridians, and because these differences were stable over time, they are interpreted as attributable to ocular factors. However, there were also slow changes with time, varying simultaneously in both eyes. These trends in differences of apparent size between two parts of the visual field can become rather large, e.g., a line 75 mm. long on one half of a meridian was seen as equal to a line 105 mm. in length lying on the opposite half of the same meridian (observation distance, 200 cm.). Visual angles for a pair of corresponding retinal points are stable and variations are small, but subjective direction of both members of a pair of corresponding points relative to the fovea can vary markedly over time. Bevan & Dukes (18) report that under "naturalistic" conditions, i.e., out of doors with stimuli at various distances and with varying backgrounds, red and yellow objects tend to be over estimated in size as compared with blue and green objects. Although the colors were distributed equally over the various positions and hence backgrounds, the backgrounds were unspecified. Brightness and color contrast presumably attending the various backgrounds should be significant. Conducting the experiment in a "naturalistic" setting makes it unnecessarily difficult to isolate significant variables. The recent formulation by Ratoosh (154) stating that cues at junction points between overlapping figures are the only determiners of perceived interposition was ingeniously tested by Chapanis & McCleary (49). They find that the over-all form of the elements is the chief factor in perceiving interposition, while only minor influence on judgments could be attributed to cues at junction points. Gerathewohl & Cibis (80) had skilled draftsmen indicate by drawings the changes they saw in size and spatial position in three dimensions occasioned in an H-shaped figure by different levels of illumination in the two eyes, and by partial shading by both fuzzy and sharp-edged shadows. Qualitative analysis of the drawings show many instances of slant and curvature in depth of the figures. The results are discussed generally in terms of stimulus and psychological characteristics relative to perception of space.

The problem of size-distance relations (size constancy) was actively investigated during the last year, but no startling findings were reported. Kilpatrick & Ittelson (112) criticize the size-distance invariance hypothesis insofar as it may be considered a description of perceptual achievement. The authors' conclusion that the size-distance invariance hypothesis is not an adequate description of perceived size-distance relations under all circumstances goes without saying. Also, their further conclusion is consistent with the current tendencies toward undue empiricism when they assert that those instances in which the hypothesis is an adequate description "... simply means that the perceptions most nearly approximate the physical situations which have been related to all the size-distance experience of the organism..." (p. 230). It is more likely that size-distance invariance describes the organism's perceptual achievement under those conditions in which the

situation provides to his sense organs, a full complement of the stimulus correlates of size and distance. Reduction of the available stimulus correlates of distance and size destroys the possibility of a unique and veridical perceptual outcome. The fact that Ss' perceptions in various reduction situations reflect expectations from past experience is an important finding, but it does not prove that all stimulus cues derive their perceptual significance through learning. Smith (175) finds that variation of distance of either the comparison or standard size stimuli has insignificant influence on judgments of size constancy. Chalmers (48) compared the effects on size-distance perception of reduced brightness of the stimuli viewed binocularly and monocularly under conditions of reduction, i.e., with most cues of distance removed. Decreased luminance shifted the binocular judgments in the direction of the monocular judgments, i.e., toward approximation of the law of visual angle, presumably because the reduced brightness decreased the effectiveness of any remaining binocular cues. On the other hand, Lloyd (127) finds brightness of stimuli to influence ratings of distance under reduction conditions. He had Ss rate the relative distance of square stimuli as a function of their area and luminance. With smaller targets (ranging from 1 to 7.3 square degrees) intensity influenced the effect of size on apparent distance much more than with larger squares ranging from 17.6 to 55.0 square degrees. The author concludes that "... where size discriminations are relatively easy, size constancy plays an important part in the judgments of distance and the effect of intensity is almost negligible. Where size discriminations are difficult, however, size constancy seems to be absent in this type of judgment and the role of intensity is greatly enhanced" (p. 182). Edwards (65) measured the apparent size of after-images projected on surfaces at different distances by matching them with a comparison stimulus viewed under conditions of reduction (no cues to distance of the matching stimulus). It is hardly surprising, as Edwards indicates, that under these conditions Emmert's law fails, i.e., projection distance does not influence the obtained size matches. S is essentially matching visual angles of the stimuli. Ittelson (107), in his reply to a paper by Hochberg & Hochberg (94), criticizing "familiar size" as a cue to distance, points out several instances of research findings which he believes indicate the importance of familiar size for judgments of distance and then criticizes the Hochbergs' experiment in which they found no influence of familiar size on perceived distance. Hochberg & Hochberg (95) reaffirm their original position.

LEARNING, MOTIVATION, AND PERCEPTION

The problem of needs and perception has generated considerable interest among psychologists, and an even greater amount of confusing semantics, in recent years. Postman (152) has contributed a clarifying and much needed operational analysis of the motivation-perception problem. He urges greater concern for operational precision in formulation and use of theoretical constructs.

Any sensory function defined in terms of discriminative responses contains some variance related to characteristics of these responses. However, when visual capacities and characteristics (perception) are at issue, factors of learning and motivation which influence the index response are usually minimized in the hope of establishing functions relating sensory characteristics to properties of the stimulus. For example, Blackwell (23) defines validity of threshold-measurement procedures in terms of their resistance to the influence of variables agreed to be irrelevant to purely sensory functions. The year's research seems to the reviewer to bolster the hypothesis that factors of learning and motivation have their primary effects on strength and availability of index responses and not on visual discriminative capacities. Postman & Conger (153) demonstrate that it is frequency of previous word responses and not frequency of experience that is basic to the effect of familiarity on word recognition thresholds. These findings seem somewhat contradictory to those of Miller *et al.* (144), who show that recognition thresholds are lower for nonsense words containing relative frequencies of letter sequences similar to those in English as compared with recognition thresholds for "words" constructed by random sampling of the alphabet. Striking evidence for the above hypothesis is provided by Lawrence & Coles (122) who find that presentation of instructions which restrict response alternatives is equally effective in reducing recognition thresholds regardless of presentation before or after the test flash. Thus, it is the responses that are "pre-tuned" by instructional set, not the sensory capacities.

Two studies measured recognition thresholds as a function of stimulus familiarity, defined in terms of controlled learning experiences. Vanderplas (180) finds that recognition thresholds are lower for nonsense material previously experienced in an "organized" manner (in the Gestalt sense) than are thresholds for less structured materials. Frequency of experience was the same for both types of stimuli. He believes the lowered thresholds are related to better response-trace availability in organized trace systems. Reece (155) reports that escape-shock reinforcement of nonsense syllables, learned as paired associates, lowers subsequently measured recognition thresholds of the response syllables to a greater degree than does nonescape shock reinforcement. The results are not very conclusive, however, because general disturbances induced by the pain-shock conditions tended to blur the supposed relationship.

Effects of memory on perceptual organization were demonstrated by Wallach and co-workers (188). They projected stationary shadows of wire figures on a screen. The shadow figures then appear predominantly two dimensional, but rotation of the wire patterns causes the shadows to appear strongly three dimensional (kinetic depth effect). At intervals ranging from minutes to weeks later, the unmoving shadow figures were again presented. Most Ss now perceived them as three dimensional.

Several studies demonstrated effects on perceptual responses of emotionally toned stimuli, but threw little light on underlying mechanisms. Beier &

Cowen (15) find poorer recognition of "threat" words than of neutral words, even if Ss are alerted ahead of time about the "threat" words. Deprivation lowers recognition thresholds for "eating" and "drinking" words, according to Wispé & Drambarean (193). Ten- to twelve-year-olds were found by Beams (13) to overestimate the size of preferred foods as compared to less-preferred foods, and according to Smith (171) "pleasant" faces are seen as larger (i.e., closer) than are "unfriendly" faces when observed under conditions of reduction on the "size-distance table" [described by Lawrence (121)].

The Lazarus-McCleary (123) concept of subception as representing a special process was viewed critically. On the basis of a statistical theory which takes account of the probabilistic nature of discrimination, Howes (101) attempts an alternative explanation of the phenomenon of subception (differential sensitivity to emotional stimuli of GSR² and verbal recognition as indicators of discrimination). Also Bricker & Chapanis (32) explain subception in terms of information conveyed to S by the stimulus, even though verbal recognition is not achieved. They find that following unrecognized paralogues, fewer subsequent guesses are required for correct identification than would be expected by chance.

Holzman & Klein (100) and Holzman (99) report evidence for the hypothesis that people have consistent response dispositions (cognitive attitudes) which are reflected in perceptual responses. They find that persons classified as "levelers" or "sharpeners" on the basis of independent perceptual tests, show differential amounts of assimilation (increased negative time error) when a dim light is interpolated between the standard and comparison lights. No difference between normal and schizophrenic Ss is reported by Knehr & Fuller (114) in estimating the number of tachistoscopically exposed dots. Characteristic individual differences were noted in patterns of under and over estimation which, however, did not seem to be related to available personality data.

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HEARING^{1,2,3}

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Because of limitations of space, a large and important area in hearing will be omitted. The anatomy and physiology of hearing has been well treated in all preceding volumes of the *Annual Review of Psychology* and the reader will not be left too far behind if he consults the excellent recent reviews of Egan (33) and Ades (1). For this serious omission, the reader must expect compensatory inclusions, and, in addition to more or less traditional areas in hearing, an attempt has been made to summarize especially the recent progress that has been made in diagnosing and treating hearing disorders. This choice not only reflects the interests of the reviewer, but also a neglected topic in previous reviews.

It is impossible to ignore the physiological aspects of hearing without at least mentioning the publication of *Physiological Acoustics* by Wever & Lawrence (133). This book is precisely titled and reports experiments having to do with the external, middle, and inner ears in so far as they deal with the process of sound transmission. The first 17 chapters deal with this transmission process up to, but not including, the receptor-transducer or the nervous system. A final chapter is concerned with the authors' evaluation of contemporary auditory theory. Two other contributions to auditory theory have come from Huggins (71) and from Zwislöcki (138). Huggins has presented a mathematical resonance theory based on analysis of signals and systems in terms of damped exponentials. His analysis in many ways appears to be more appropriate to auditory function than does the more traditional sine-wave analysis, which is better suited for the steady-state. Zwislöcki (138), concerned primarily with cochlear mechanics, has summarized the contributions of and differences among several contemporary theorists. He suggests a convergence appearing from these several views, but points out that the different theories explain different facts.

AUDIOMETRY

In its broadest sense, audiometry concerns all of auditory psychophysics but has come to be restricted in its meaning to measures that relate individuals to a norm with respect to any of several facets that have to do with

¹ The survey of the literature pertaining to this review was completed in May, 1954.

² The following abbreviations are used: GSR, galvanic skin response; DL, difference limen; cff, critical flicker frequency.

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hearing disorders. Clinical audiometry is no longer concerned only with the audibility of pure tones; rather it includes also the perception of speech, differential sensitivity, masking, fatigue, and even psychological magnitudes like loudness and pitch. Several general books have been written on these several aspects by Fournier (37), Hirsh (66), Kietz & Zangemeister (79), Langenbeck (81), and Watson & Tolan (131).

The normal threshold.—So long as there was only laboratory interest in the auditory absolute threshold, no one was much concerned about specifying a "normal threshold." But the advent of pure-tone audiometry and its widespread use in clinical settings has led to a demand for expressing such threshold measurements relative to a standard norm. The history of the present standard began when Beasley (10) reported the results of a U. S. Public Health Service Survey in terms of attenuator readings and voltage calibrations from the Western Electric 2-A Audiometer. The earphone was the Western Electric 522. No American standard has yet been adopted or proposed which is written in terms similar to those of laboratory studies, namely sound pressure in the ear canal. The present American Standard (3) is expressed in terms of the sound pressure developed in a standard 6-cc coupler by a laboratory standard Western Electric 705-A earphone. The pressures were obtained by balancing the loudness produced by this standard phone and that produced when certain fixed voltages, known from the Beasley study, were applied to the 522 earphone. Similar loudness-balance procedures have been used by the National Bureau of Standards to include, in the standard, other earphones that might be used by audiometer manufacturers [Corliss & Burkhard (23)]. Such a subjective procedure is required because earphones with different internal impedances respond differently when placed on different ears, and sound pressure produced in a standard coupler is not a valid indicator of the pressure that would be developed near the eardrum of even a single observer for several earphones. Corliss & Burkhard (23) have demonstrated (almost) that the measurement of sound pressure at the entrance to the ear canal can be used as a valid correlate of whatever the appropriate measure of stimulus intensity might be, thus suggesting the substitution of a physical procedure for a subjective one. They write "... the sensation of equal loudness corresponds directly to equal sound pressures at the entrance to the ear canal. This is a fundamental assumption upon which threshold transfer by loudness balance is based." This reviewer believes that this is a conclusion, not an assumption, that Corliss & Burkhard arrive at from data that are more suggestive than conclusive. Actually, the loudness-balance procedures are used because this statement has not yet been adequately tested.

The physical specification of the American Standard is another matter. Munson's measurements (122) of the pressure developed in the ear canal under one of the 522 earphones that was used in Beasley's survey were used by Steinberg & Gardner (122) and later by Steinberg, Montgomery & Gardner (123) for expressing the Beasley (10) data in terms of ear-canal pressure.

These secondary sources, not the Beasley data and not the American Standard, have been used by Dadson & King (25) to show a discrepancy between American and British thresholds. Dadson & King (25) measured the thresholds of 99 young adults in terms of voltage applied to still another earphone. These voltages were converted to ear-canal pressure measurements, made on 20 ears. Wheeler & Dickson (134) used the same phone to measure thresholds of another 1024 ears, and the two studies not only agree with each other but also with the estimated minimum audible pressures according to Sivian & White (120). But between these three studies and the American Standard as interpreted by Steinberg & Gardner (122), there exist differences as large as 20 db at some frequencies. It still remains for some one to determine the ear-canal pressures, and, more hopefully, pressures near the eardrum, that correspond to the American audiometer standard.

These and other problems, particularly with regard to a possible international standard, have been considered by Lehmann (84), who reviews most of the important variables. He seems more concerned with the difference between earphone and free-field thresholds and goes into the matter in some detail. He also provides, in a single source, drawings of the couplers used in the United States, Great Britain, France, and Switzerland.

Bone conduction.—For many years otologists have been able to distinguish between certain auditory pathologies of the external and middle ears and those of more central origin by comparing the auditory threshold for sounds transmitted through the normal air-conduction route with that for sounds conducted by way of the bones of the skull. The classical tests for such diagnostic information employ tuning forks, but more recently the electrically excited bone-conduction vibrator of the pure-tone audiometer has been used. If the normal threshold for air conduction presents a problem, certainly a similar standard for bone conduction is even more difficult to state. Work continues at the National Bureau of Standards (97) and at other laboratories on measuring the mechanical properties of the mastoid and forehead regions of the average head, with the view to establishing an artificial mastoid or headbone that would serve for these threshold standards in the way that the standard 6-cc coupler has served for air-conduction measures. Lehmann (84) opines that subjective methods must continue to be used in this area. One of the chief variables in bone-conduction audiometry, the force of application of the vibrator to the skull, has been studied by Harris, Haines & Myers (62). They show that reliability improves when a helmet-held vibrator is applied with constant force, as compared with a hand-held vibrator. They indicate, however, variations in threshold of only 2 or 3 db with vibrators held in the hand by even inexperienced observers. The threshold for 250 c.p.s. varies with forces ranging from 100 to 400 gm., but for all higher frequencies only the variability changes, being reduced as the force of application is increased.

On the basis of inferences drawn from experiments and a mathematical theory of cochlear mechanics, Zwislocki (140) points out that hearing by

bone conduction introduces nothing new or different from the more usual air conduction, in so far as cochlear activity is concerned. We are dealing only with a different means of transmission, the end result being handled by the cochlea in much the same way as obtains for sounds delivered by way of the eardrum-stapes system.

Theories of bone conduction have been reviewed by Fournier (39), who holds that clinical usefulness of bone-conduction results has outrun theory by many years, and there remain several clinically demonstrated phenomena that are unexplained by the widely accepted views of Bárány and Békésy [see 66, pp. 242-53; (39)]. Occluding the ear of a normal listener, for example, lowers the bone-conduction thresholds for that ear, but this is not to be confused with the lateralizing toward the ear with conductive hearing loss observed with patients (39). Everberg (34) points out that the occlusion of a normal ear results in an absolute improvement of bone conduction while a unilateral conductive loss produces only a relative improvement for the affected ear, depending on the noise level in the room to which the normal ear is exposed. Many of the general rules concerning the masking of pure tones by noise may not apply to certain clinical cases, for example in testing a unilateral pure conductive loss when it is desired to prevent the better ear from detecting bone-conducted sound [Fournier (40)]. Only a portion of these reports by Fournier are available in a shorter English article (41).

Otosclerosis, one of several causes for conductive hearing loss, has utilized bone-conduction results both for diagnosis and for estimation of success or failure. The fenestration operation, of course, is supposed to alleviate much of the air-conduction hearing loss. A new report, not alone in its claims, has been published by Henner (65) which indicates that bone-conduction thresholds are also improved in certain cases.

Speech audiometry.—Although one of the purposes of pure-tone audiometry has been to replace the old-fashioned spoken and whispered speech tests, more recently speech has returned to audiometry, this time as a moderately well-controlled stimulus (68). Many of the concepts that were developed in connection with evaluating communications equipment have been taken over and at least two kinds of clinical measurement have become almost classical (66, Chap. 5). Similar to the audiogram, for at least the middle frequencies, is "hearing loss for speech," or the difference between a patient's threshold and a normal threshold of intelligibility for speech. The second measure has to do with "discrimination loss" or the difference between the highest intelligibility score that a patient can reach, no matter what the intensity, and the normal maximum, about 100 per cent. One has only to point to several monographs to note that these concepts have indeed become general and furthermore have become applied in French (38), Swedish (86), Finnish (99), and Danish (111), to mention only the most comprehensive works. All four of these references present methodology, description of equipment, derivation of the speech material, and considerable case ma-

terial illustrative of the kinds of interpretations and conclusions that the technique affords.

Pronovost & Dumbleton (109) and Sortini & Flake (121) have described modifications of speech audiometric tests for young children in which the appropriate response is either picking up or pointing to an object named, rather than a verbal response. The former appears to be fairly analytic with respect to specific speech sounds missed, while the latter employs a fairly limited number of words, primarily for threshold use.

"Objective" audiometry.—Most procedures in speech and pure-tone audiometry require either that the patient make a verbal response, that he follow the verbal instructions of the tester, or both. These requirements are not applicable to persons, especially children, who have not acquired language nor to persons who are suspected of malingering. Although nonverbal, "objective" procedures have been sought for a long time, an accelerated growth in such techniques can be seen since 1948, when Bordley, Hardy, & Richter (17) reported their success in conditioning the galvanic skin response to a sound. The sound could be weakened until the conditioned response was no longer obtained and thus a threshold technique was available. An experimental study by Wilcott (135) indicates that subthreshold conditioning by this technique is not possible. He measured a key-pressing threshold for three tones and white noise and then, having established GSR² conditioning at and above that threshold, attempted conditioning at the highest intensity for which the listener never pressed the key. No evidence for GSR response to these just subliminal sounds was obtained; it appears that thresholds for both responses are the same. Goodhill, Rehman & Brockman (52) indicate, on the basis of examining 150 patients, that the clinical usefulness of a threshold for GSR conditioning requires and is enhanced by thresholds from other procedures for comparison. Portmann & Portmann (107) express the same view after a series of tests on 500 children and insist that a valid clinical picture depends upon the use of several different threshold procedures. Barr (9), however, prefers the GSR to play audiometry, at least for very young children. Goldstein, Ludwig & Naunton (50) used this same technique for a population of 42 deaf and aphasic children from two schools for the deaf. They emphasize not so much the threshold obtained as the way in which the conditioning proceeds, and they observe that those children independently judged to be aphasic, or at least judged to be different from deaf children, are much more difficult to condition than the deaf group because they give relatively unreliable responses. These authors suggest that the development of the conditioning itself may be as, or more important, diagnostically than the actual threshold obtained.

Maspetiol, Pegement, & Tronche (90) have employed an unconditioned GSR to loud sounds to define a "threshold of uncomfortable hearing." This threshold for eliciting the unconditioned GSR runs from 30 to 40 db below thresholds associated with actual pain. These authors report, however, that

this autonomic threshold is relatively easy to obtain and defines for them a ceiling on the auditory area which, when compared with the absolute threshold curve, gives information about the dynamic range of the ear. In some ways these authors consider this range to be analogous to the one determined by recruitment tests (see below).

Other responses, like the startle, have been used as qualitative indicators of hearing. Goodhill (51) has reported similar observations in a sound-treated room with loudspeakers in each corner and with a variety of sounds under good control. He thus obtains a semiquantitative threshold for startle, as opposed to the older, more usual yes-no observation in an uncontrolled acoustic environment. Galambos, Rosenberg & Glorig (45) have described a method in which a subvert eye-blink response to an acoustic click is obtained by means of a phonograph cartridge and oscilloscope. This very stable component of the startle response is elicited by clicks about 90 to 100 db above threshold, a level quite similar to the threshold of unconditioned GSR (90). In cases of moderate hearing loss the threshold is obtained at a correspondingly higher level, and the curve relating response to intensity climbs more steeply than in the normal. The test does not clearly differentiate organic from nonorganic or conductive from "nerve-type" hearing loss, but the authors suggest that it can be used in detecting certain types of malingering. Jepsen (73), in discussing several unconditioned responses to sound, describes a technique for measuring a change in the acoustic impedance of the ear that corresponds to an intratympanic reflex. The application of this complicated procedure appears to be the same as for the simpler observation of startle-response components in cases of severe hearing loss or deafness.

For the past several years studies have been made on the effect of hearing one's own voice when the sound delivered to the ear is delayed by different amounts of time. In general, such a delay produces an increase in loudness of speaking, a slowing down and prolongation of sounds, and sometimes stuttering. Because of the rate at which informal "grapevines" among laboratories and clinics transmit information, it is difficult to assign priority to the application of this phenomenon to testing for malingering in hearing. Clearly, a person who feigns deafness will not be successful if he is asked to continue talking while his speech, amplified and delayed, is presented to his ears. Heller & Lindenberg (64) have discussed such an application along with the use of GSR conditioning in a paper on the evaluation of deafness of non-organic origin. Atkinson (4) has reported a further experimental finding in connection with this delayed side-tone to the effect that talkers do not adapt to this time-delay after continuous reading for several minutes. He suggests that even longer reading times might show some adaptation or return to more normal talking.

Screening.—As in other sensory modalities, so in hearing, it is sometimes useful to know simply whether or not a person or a group can respond to a certain stimulus level. Thus, many techniques for screening military, industrial, and school populations have become available. One of these, the

Massachusetts Hearing Test (77), has been evaluated by DiCarlo & Gardner (28) on a group of college students. This test, originally designed for younger school children, appears to be valid for a college population when validation is measured in terms of individual pure-tone audiograms. The advisability of similar screening in the primary grades has been demonstrated by Wishik & Kramm (136) who report the results of a biennial testing program on 1726 children. About half of the children who failed the first screening test gave clinically satisfactory audiograms, and then among 74 children whose audiograms were unsatisfactory, all but one had confirmed hearing impairment after otologic examination. The percentage of children who significantly failed the test at one time or another varies from about 2 to 6 per cent, depending on the grade and testing criterion. With an individual pure-tone audiometer, Falbe-Hansen (35) studied the hearing of children in the schools of Copenhagen. He reports somewhat higher incidences of hearing loss: 15.5 per cent from relatively poor residential sections and 9.7 per cent from better sections. Of those found defective, 22 per cent sustained perceptive losses while 78 per cent had conductive impairment. Otologic and other treatment produced an improvement in 90 per cent of the cases.

DIAGNOSIS AND TREATMENT OF HEARING DISORDERS

It would be impossible and pretentious to attempt to review all the recent work having to do with the medical, prosthetic, and educational aspects of hearing disorders. On the other hand, it seems worthwhile to emphasize the contribution of psychophysical tests to these areas. Certain fundamental relations, like the comparison of bone with air conduction, are so well established that recent work on them has not been reported. The most important recent contributions of audiometry to diagnosis have to do with distinguishing between pathologic conditions of the cochlea and those of the auditory nervous system.

Recruitment of loudness.—Briefly stated, auditory recruitment is present when the loudness, associated with given decibel increments above the threshold, increases more rapidly than normal. The descriptive details of this phenomenon as well as procedures used to detect it clinically are available in the excellent reviews by Harris (57) and Egan (33). For many years recruitment was used clinically to make more firm the distinction between conductive and nonconductive hearing loss. But, in 1948, Dix, Hallpike & Hood (32) observed that in a population of nonconductive hearing losses, recruitment was found only in cases of Ménière's disease, commonly associated with disorders of the end-organ, but not found in cases with tumors of the eighth nerve and nearby nerve centers. These observations have been reviewed and elaborated more recently by Dix & Hood (31), who present observations on two auditory-nerve tumor cases with recruitment, which are exceptions to the earlier rule. Surgical removal of the tumors did not change the hearing loss at threshold significantly, but the recruitment disappeared. The authors state, therefore, that these cases were not true exceptions but rather that

these particular tumors involved the cochlear blood supply and thus indirectly the end-organ. Dix & Hood recommend recruitment tests employing only short tones because it appears that a recruiting ear is also more susceptible than normal to short-term auditory fatigue. They argue that many of the works that have failed to substantiate the claims of the 1948 paper have employed tones that were too long and thus the apparent increase in loudness that should have been obtained was obscured by fatigue which not only reduces the loudness to normal, but also to subnormal.

The fact that a recruiting ear is abnormally susceptible to fatigue was demonstrated earlier by de Maré (89) and by Hood (69). Pestalozza (102) has further substantiated these claims and shows how adaptation tests may themselves be used to ascertain the same pathology as does a recruitment test. Although he does not consider the adaptation and the recruitment to be due to the same underlying mechanism, van Dishoeck (30) indicates that both are present in the same ear. In this same article he reviews and attempts to distinguish among the terms masking, fatigue, adaptation, and recruitment as applied to diagnosis.

Indirect procedures.—Among the techniques for measuring recruitment in which the proponents have sought to avoid loudness judgments are several new ones. Miskolczy-Fodor (93) has proposed a technique for measuring a threshold in terms of the duration of a tone whose intensity is from 3 to 6 db above the long-tone threshold. It is claimed that this minimum impulse time can be converted to loudness. Bangs & Mullins (6), after reviewing other techniques, suggest that a range-of-loudness method be used which establishes the difference between a "most comfortable level" and an "uncomfortable level." In spite of the apparent validity which comes from an adequate separation of categories of patients, an assumption remains untested, namely that levels that yield equal annoyance and/or equal discomfort are the same as levels that yield equal loudness. This same assumption is also involved in the claim by Saltzman & Ersner (116) that something like recruitment appears at high intensities in cases of otitis media. But the role of a diseased middle ear in producing an increased excitability in pain-nerve endings in the vicinity of the eardrum is not well enough differentiated by these authors from increase in loudness. When a listener objects to a strong tone, it may be because the tone is painful or too loud; these two reasons are not the same.

On the assumption that when loudness increases more rapidly than normal the difference limen for intensity should be smaller than normal, and on the further assumption that since the DL^2 decreases with increase in loudness it should be smaller at comparable sensation levels for recruiting ears where the loudness is higher than normal, Lüscher & Zwislocki (88) proposed several years ago that the intensity DL could be used as an indirect measure of recruitment. Jerger (74, 75) has reported that the DL measure is more significant clinically if the difference between the DL at 40 and at 10 db sensation levels is used rather than the DL size itself at any particular level. The relation between the size of the DL and pathological loudness

functions associated with recruitment remains somewhat obscured by the variety of measurement techniques that have been employed in different clinical reports.

A further technique, based on masking, for distinguishing between lesions of the cochlea and those of the auditory nerve was proposed earlier by Langenbeck [(81) Chap. 7]. Palva, Goodman & Hirsh (100) tested this procedure on a sample of 90 ears, including a variety of diagnostic categories, and found that the threshold for pure tones at octaves from 250 to 8000 c.p.s. in the presence of 100 db of white noise (20 to 7000 c.p.s.) were almost the same (within a range of 10 db) for all groups. They concluded that the masked threshold for pure tones in the presence of white noise is not a useful indicator for differential diagnosis.

As was mentioned earlier, in the section on bone conduction, masking is also used clinically to prevent one ear from hearing while the other is being tested. Burgemeistre (19) and Fournier (40) have reported on procedures for such use of masking noise for both air-conduction and bone-conduction tests in a variety of patients. Zwislöcki (139) has contributed to this complicated problem with a study on the acoustic attenuation between the ears. His results indicate that bone conduction plays a large role in the transmission to the opposite ear, from that to which the sound is delivered, a finding that is different from an earlier one of Békésy (11). One further clinical use of masking and fatigue tests is reported by Sambataro & Pestalozza (117), whose results on the masking and fatigue effects of noise on speech thresholds indicate that normal listeners are more susceptible to such effects than are otosclerotics. They interpret their results as supporting a hypothesis that fatigue of the stapedius muscle is involved.

Contributions of speech audiometry.—The main conclusion to be derived from a number of studies employing speech audiometry in relation to diagnosis is that a substantial discrimination loss (see above) is usually associated with the same kind of pathology as that found with recruitment of loudness (66, 86, 99). Similar behavior is obtained by Simonton & Hedgcock (119) when the speech is presented against a background of noise. In a way these important findings on the relation between speech discrimination and diagnosis add nothing remarkably new to the diagnostic picture except perhaps to suggest themselves as nearly adequate substitutes for recruitment tests. They do make much clearer, however, the implications of this type of pathology for the use of hearing aids, whose only function is to amplify sound, since intensity increase alone cannot assist greatly in these cases.

Surgical treatment.—The chief surgical technique that has been reported in the recent otological literature has been the fenestration operation, in which a new window is created as a substitute for the oval window with an ankylosed stapes, a condition found in otosclerosis. The main contributions of audiometry to this surgery lie in two areas. First, before the otologist selects a patient for this operation he needs to have some assurance that the transmission loss of the middle ear is the chief if not the entire component of the total hearing loss. Cochlear involvements can pretty well be ruled out

by recruitment tests or speech-discrimination tests. Second, the procedures of pure-tone and speech audiometry afford convenient frameworks in which to report the results of fenestration surgery. An example of such reporting is found in an article by Farrior, Bagby & Thomas (36). The study of the progress of changes in hearing after operation are also thus reported by Zanotti (137) and by Henner (65). Audiometric findings were used by Rosen (112) as evidence for the validity of a procedure for otosclerosis in which the surgeon merely manipulates, wiggles, or otherwise "unfreezes" the stapes.

Medical treatment.—It does not seem out of order to claim that the diagnostic decisions based on recruitment and other associated tests over the past several years have permitted otologists to discover more rapidly or at least to be more sure of cases of Ménière's disease. Hence, we find several articles on the treatment of this disease, proportionately more than in earlier years. Atkinson (5), Fowler (42), and Juers (78) have reported on chemotherapeutic procedures; Johnson (76) has reported on a surgical procedure; and Derlacki (27) has reported on psychotherapeutic procedures.

Hearing disorders in children.—Here we touch another area that occupies whole professional groups of clinical workers and teachers. Obviously, the detection of serious hearing loss in young children is extremely important for the proper selection of special educational programs. A general treatise on the problems that surround such early detection, and, in particular, those problems concerned with differentiating hearing disorders from mental and other language disorders, has been published by Myklebust (95). Related concepts are stated in abbreviated form in an article by Pauls & Hardy (101).

Most educators agree that at least two alternative programs be considered for a hard-of-hearing child: special classes in ordinary schools or special schools. Those whose hearing losses are moderate enough to permit attendance at ordinary schools usually have added to the curriculum special auditory training classes, roughly the auditory analogue of remedial reading. Children may use individual, wearable hearing aids or group hearing aids, two varieties of which are described by Hudgins (70) and by Bangs & Shapley (8). The emphasis appears to be on more or less constantly amplified sound so that cues for speech discrimination are available for the training process [Bangs (7)]. The evidence reported by Wedenberg (132), Hudgins (70) and Irwin & Shreve (72) indicates that even lipreading is enhanced by the auditory counterpart and vice versa.

Wedenberg's study (132) covers approximately 14 years of experience with 36 children, falling into several categories according to audiometric results. His emphasis has been on auditory training with the visual part of the training held up until after auditory discriminations within the ability of the child have been established. The progress of even his most profoundly hard-of-hearing cases is indicative of a highly successful program. Hudgins (70) has reported a similar program within the framework of a school for the deaf where he worked with two groups of children, one subjected to well-

controlled acoustic stimulation and the other subjected to only the ordinary kind of training, with relatively poor electroacoustic equipment as an adjunct. His results, mostly concerned with profoundly deaf children, show the advantages of good electroacoustic equipment on the ability of the children to discriminate words spoken by others, not only when heard, but also when either only seen or when seen and heard together. Similar evidence for the cooperation of the two senses in the training of deaf children comes from Leonardelli (85), who studied 21 children with hearing losses ranging from moderate to severe. He states that when either lipreading or hearing with amplification appears insufficient for understanding spoken language, the two used and trained in cooperation will permit conversational comprehension. The advantage becomes less as either ability alone decreases. Methods used for assessing the speech, hearing, lipreading, and other abilities are found in Hudgins (70), Wedenberg (132), and Bjuggren (15).

THE NOISE PROBLEM

During the past year, two important practical issues have accelerated the interest of both the public and some scientists in the problem of noise. First, it appears that very large sums of money are involved in potential compensation cases arising from the loss of hearing due to noisy working environments [Fox (43)]. Second, the noise levels encountered at air bases and on the decks of aircraft carriers in the vicinity of modern jet aircraft with afterburners are much higher than any sound levels that have been coped with in the past [BENOX (13)]. As a result of the demands stemming from these two situations, several summary documents have become available which bring the appropriate scientific information together and up to date, and which outline directions for further research. Loring (87) has published a bibliography on the effects of noise on man, which lists 741 references in this field. It brings up to date and extends the coverage of a previous review monograph on this subject by Kryter (80).

Industrial noise.—Rosenblith & Stevens (113) have written a volume that contains, in addition to a brief and excellent short course in psychoacoustics, a review of information available on the effects of noise on human behavior. The chief three categories into which this material is divided have to do with the annoyance of noise, the effects of noise on hearing, and the effects of noise on biological structures and nonauditory human behavior. In a final chapter they interpret the results of their summary by listing tentative criteria for noise levels by octaves, for both speech interference and damage-risk.

The American Standards Association has indicated a desire to set up criteria for noise control by appointing a subcommittee on the subject. Rosenblith, Rudmose *et al.* (115) have published a first report from this subcommittee indicating that not enough information is yet available for fixed criterion levels. This report provides, however, considerable progress toward specifying the relations between the actual hearing loss in a group of work-

ers and the level and type of noise to which they have been exposed, as well as the number of years over which exposure has gone on. This report also pulls together information on auditory thresholds, as a function of age, from three separate surveys among which the agreement is considerable. The authors of the report thus feel justified in publishing smooth curves that relate hearing loss, for different frequencies, to age in years. They propose that such information be used to correct the gross hearing loss found among workers who have been exposed to noise in order to ascertain, on the average, the net hearing loss that may be attributed to the noise itself. A detailed analysis of the measurements from one industrial plant, which included both reliable audiograms on workers and specifiable levels for continuous noise was made; it permits reasonable prediction of hearing loss at frequencies 1000, 2000, and 4000, based on the level of noise in the best predicting octave and on the exposure time in years. Their trend curves are extremely useful, particularly if supported by further study. Sataloff's results (118), for example, are predicted by these trend curves reasonably well. A further conclusion in this report by the ASA subcommittee Z-24-X-2 is that intermittent exposure to noise is much less damaging than continuous exposure. This conclusion is based on a study of the hearing of airplane pilots. More rapid intermittency also favors the preservation of hearing, as is noted in studies on the hearing of riveters, although the specification of noise levels of the impulsive type is difficult and not easily comparable to levels associated with continuous noise. The impact noise of the drop forge, for example, results in considerable hearing loss [see also Fox (44)].

In a study on 154 employees of a jet-engine test unit, Sataloff (118) finds relatively little hearing loss for periods of exposure up to five years. He concludes that the ear is quite resistant to noise trauma, but it should be noted that the longest exposure per day among his experimental group was about two hours. Larsen (82) provides a brief review of some of the clinical literature on industrial noise and illustrates also the difference between the hearing loss of a riveter measured immediately after work and measured again the following morning after a night's rest. The gain was about 45 db, an amount that emphasizes the necessity of waiting for some time after the most recent exposure before assessing the permanent component of hearing loss. Similar results on a larger population are found in the report of Cox, Mansur & Williams (24), who show that hearing losses sustained by workers in the weaving and spinning rooms of a textile mill are significantly greater when measured 15 min. after work than when measured on Sunday evening, when more than 40 hr. had elapsed since the last exposure to working noise.

Larsen (82) also states that the validation of short-time fatigue tests as predictors of susceptibility to permanent hearing loss from noise has yet to be made. One more test has been proposed for predicting the susceptibility of individuals to acoustic trauma. Lawrence & Blanchard (83) start from the assumption that the sooner a mechanical system begins to distort, as the input is increased, the sooner it will break down. Thus, an ear susceptible

to acoustic trauma should have a relatively low threshold for the onset of nonlinear distortion. A first experimental step shows that there is a fairly wide range of thresholds among human listeners with respect to the threshold for detection of the second harmonic of a fundamental by the method of beats. Similarly, five guinea pigs show different thresholds for a departure of the input-output function for cochlear potentials from linearity. The "validating" step shows that those guinea pigs whose output functions depart from linearity at the lowest intensities are the ones that sustain the greatest hearing loss when exposed to an intense tone. The experimental evidence is suggestive for a validating study on humans, but some of the unexplored assumptions implicit in the rationale for such a test remain to be proven.

Perhaps the sharpest focus of the industrial-noise problem is to be found in the aircraft industry which is not only large but also noisy. At a recent meeting of the Acoustical Society of America a whole symposium was devoted to aircraft noise, mostly physical studies of its production, measurement, and control. The first paper at this symposium summarizes the area quite generally [Bolt (16)]. People are involved in three locations: in the airplane, in or on the airport, and in areas neighboring the airport. Problems range from possible hearing damage, through communicative masking to annoyance.

Jet-aircraft noise.—When the Navy began to use jet aircraft with afterburners on carriers, problems that had been mounting in severity over the past several years reached a climax. It was not simply another case of exposing men to high-level noise; rather, the noise levels exceeded those in which auditory scientists from both the physical and biological sciences were accustomed to work. With the human threshold for pain from acoustic stimulation around 135 db, these new levels to which men were exposed exceeded 140 db by significant amounts. A group of scientists representing a variety of academic disciplines was asked to prepare "An exploratory study of biological effects of noise." Now published, it is known as the BENOX Report (13). Since many of the investigations and factfinding summaries are of a preliminary nature, we shall not consider the several chapters in detail; instead, we shall note only the outstanding topics. The problem is summarized, and recommendations for the future are made by Davis (26) in the first chapter. Neff (98) points out that jet-noise levels exceed the damage-risk criteria of Rosenblith & Stevens (113) by 20 or 30 db, though there is no direct evidence yet on hearing loss from jet noise. Ear protectors seem to effect some reduction in temporary hearing loss following relatively short exposures. A chapter by von Gierke & Warren (49) points out that the theoretical limit of earplug protection, set mostly by the shear compliance of the skin of the ear canal, below 1000 c.p.s. is about 26 db. Furthermore, it appears that some presently available earplugs almost attain this limit. Ades (2) reports that the threshold for stimulating the vestibular system by sound is about 135 db in the middle frequencies and, thus, jet noise can directly affect this system. Some postural difficulty, apparently not involving

the vestibular system, seems to accompany exposure to more than 140 db. Ward (129) suggests the possibility that the reticular formation may be stimulated and thus contribute to more general symptoms of the central nervous system, particularly changes in certain deep tendon reflexes. Complaints of tiredness, irritability, insomnia, and "loss of libido" are not accompanied by objective evidence for physiological fatigue, but lead Halstead (54) to hypothesize a marginal stress syndrome that is reflected in tests of higher brain function. It is clear from these reports that the effects of jet noise on man must now be studied by persons interested in areas beyond those of the auditory system.

INFORMATION IN AUDITORY PERCEPTION

The development of information theory has provided psychology not only with the theory itself but also with a powerful mathematical tool for analysis and design of experiments. The basic concepts are available in an easily readable form in Miller's discussion (92). Briefly stated, an amount of information can be calculated from the number of alternatives among which an observer must select in order to respond appropriately. If we ask a student to select one of four alternatives on a multiple-choice question, the correct choice carries 2 bits of information, namely, two successive choices between two alternatives. (This oversimplification assumes that all four alternatives are equally probable—a condition that is usually avoided in constructing such questions.) Experiments involving absolute judgment lend themselves well to this type of analysis since the observer's responses are determined in part by the number of alternative stimuli among which he must choose. Garner & Hake (48) have given a general treatment of this particular application of information measurement. This application has been made in a number of recent studies on absolute judgment, or identification.

Absolute loudness judgments.—Classical studies on the difference limen for intensity indicate that there are many discriminable steps between the absolute threshold and the threshold for pain. But, suppose we present tones one at a time and ask the observer to identify, or put a number on each. How well will he do? Garner (46) has attacked this question directly by asking his observers to identify each of several tones, all at the same frequency, separated in intensity. Listeners can identify different loudnesses precisely when only four or five different tones make up the entire set, but when the number of alternatives is increased above five, errors begin to occur. When 20 different loudness categories are used, for example, the errors are so great that the information received per tone is only about 1.6 bits, an amount that would be received had each of only three categories been identified without error. When due allowance is made for the intersubject and day-to-day differences, the amount increases to that equivalent to perfect identification among about five categories. Information transmission is higher when the tones are separated by equally discriminable steps than by equal decibel

steps. It appears that the errors committed by observers place a ceiling on the number of loudness categories that can be accurately identified at about six, or a transmission of about 2.5 bits per tone.

Absolute pitch judgments.—Pollack (103) reports a similar figure of five pitch categories, corresponding to a transmission of about 2.3 bits per tone, when observers are asked to identify each of eight tones, equally loud but separated into equal log-frequency intervals from 100 to 8000 c.p.s. Had the observers identified each tone correctly, the information per tone would have been three bits,—i.e., three successive binary decisions. Holding the frequency intervals constant and increasing the frequency range so as to provide 9 and 11 categories, Pollack (104) also reports information transmissions of 2.2 and 2.3 bits, respectively. In this second study it was shown that the inclusion of certain fixed and labelled reference tones improved the accuracy of identifying tones near the reference tone in frequency, but this maneuver did not improve the over-all transmission rate remarkably.

Somewhat lower amounts of information are reported by Hartman (63) who asked four different groups of observers to identify each of nine tones, separated by pitch intervals of 50, 100, 200, and 300 mels. With seven weeks of practice, listeners of the 300-mel separations improved from an information reception of 1.3 to 2.3 bits per tone. Again it is to be noted that the equivalent number of categories that would have been identified correctly is about five, even after the error scores have reached something of a plateau for the given nine categories. Smaller pitch intervals with the same number of tones do not provide as good identification; for 50-mel separations the information reception rose from 1.1 to only 1.4 bits per tone over the same practice period.

If a listener has "absolute pitch" for all of the musical notes say on a piano, then he should be able to identify correctly each of these tones, by definition. This would correspond to an information transmission of more than six bits per note for 88 notes. Ward (130) reports that one such listener could consistently identify 70 different tones within the range 50 to 4500 c.p.s.; a transmission of more than six bits per tone! Informational analysis seems to substantiate a real difference between absolute pitch and the more usual pitch identification of between two and three bits per tone.

Absolute bidimensional judgments.—It appears that if a group of tones is differentiated on the basis of one dimension only, such as loudness or pitch, the listener can identify accurately each of five or six alternatives. But clearly human listeners can identify correctly many thousands of different sounds, sounds which must be different from each other with respect to many more than one dimension. Pollack (104) has shown that the information per tone carried in a display can be increased considerably by combining the dimensions of pitch and loudness in making the tones different. When listeners were asked to differentiate tones with respect to loudness and pitch separately, the amount of information per tone was 1.3 bits for intensity and 1.6 bits for frequency with five categories for each dimension. When the

stimulus patterns themselves are identified, with the loudness and pitch combined, the information is about 3.1 bits per pattern, which is higher than the 2.9 bits that represents the sum of the individual-dimension information transmissions.

Absolute multidimensional judgments.—If each dimension alone can be accurately separated into five or six categories, then how many different dimensions can be handled simultaneously? And as we approach a limit, can we increase the number of dimensions and decrease the number of alternatives per dimension and achieve still higher rates of information transmission in audition? These are the kinds of questions to which Pollack & Ficks (106) have addressed themselves. They compare performance in identifying members of two auditory displays: in one, sounds were differentiated with respect to eight dimensions each of which took on one of only two values, while in a second, six dimensions were employed with five values in each. The authors went outside of traditional auditory attributes to include such dimensions as right or left localization, interruption of noise, etc. Not all of the dimensions are equal with respect to ease of discriminability, but the over-all procedure appears to provide the listener with more easily identifiable categories. We are dealing with information reception of the order of seven bits per sound, as contrasted with 2.3 bits per tone when even large numbers of categories are employed for a single dimension. In this experiment the average information transmission for eight, 2-value dimensions is about the same as for six, 5-value dimensions.

PERCEPTION OF SPEECH

Students of speech and speech perception will welcome Carroll's book (21) in which the science of linguistics is related to the several other disciplines that are concerned with speech and language. The bibliography and the interrelations among fields are particularly useful for reference purposes.

Recognition of speech sounds.—Harris (55) reports experiments in which the steady-state portions of arbitrarily defined phonemic units are cut out from a tape on which normal speech was originally recorded. He has attempted to find out how many variations in such units are necessary to produce intelligible speech. The steady parts of traditionally listed English phonemes are not sufficient for intelligibility and it is concluded that certain transition properties must also be used. Tiffany (126) has added information on the properties of isolated vowels that contribute to their recognition. A panel of 18 listeners identified each of 12 different vowels spoken in isolation by 4 different talkers. Significant variation in recognizability is found when duration, onset characteristics, and inflection are varied. Vibrato contributes to recognizability as does including the vowel in a "t-p" consonant environment. Naturally "short" vowels are recognized better at shorter durations, while "long" vowels are better identified at longer durations.

Speech thresholds.—When the level at which speech is presented to a listener is varied until the listener reports that he "hears something" or can rec-

ognize the words 50 per cent of the time, then the thresholds of detectability and of intelligibility, respectively, have been measured. Tiffany (127) has added further evidence for the conclusion that a threshold of intelligibility for sufficiently easy speech material is primarily dependent on a threshold of detectability for the component sounds. The threshold for detecting sustained vowels lies about 10 db below that for the intelligibility of two-syllable spondee words, but apparently measures about the same thing. Tolhurst (128) has reported that the thresholds of detectability for eight individual consonants are higher than that for a single vowel. This threshold is close to that for pure tones in the middle-frequency region and lies somewhat below the threshold of intelligibility for spondee words, the difference being termed by Tolhurst an "aphonemic interval."

The masking effect of white noise and of 11 bands of noise, each corresponding to a pitch interval of 250 mels, on the threshold of intelligibility for spondees has been studied by Hirsh & Bowman (67). Their data for white noise agree well with older studies while the data on masking by bands indicate that the threshold is elevated most by noise frequencies close to 1000 c.p.s. Furthermore, the function that relates masked threshold to the level of masking noise is linear for white noise and for the more effective middle-frequency bands, but is curvilinear for bands of noise at either end of the audible frequency range.

Speech analysis and synthesis.—Much of the already familiar analyzing apparatus for speech is noted by Carroll (21). Benson & Hirsh (14) have extended measurements on the long-time average spectrum for English speech. Three different sampling times, three kinds of verbal material and 10 talkers, five men and five women, all yielded similar results except for a systematic difference between the men and women in the lowest octave measured, 75 to 150 c.p.s.

Recognizable vowels have been produced by an electrical analogue of the vocal tract, described by Stevens, Kasowski & Fant (124). This analogue represents 35 cascaded sections of the vocal tract, each $\frac{1}{2}$ inch long and each having a cross-sectional area variable from 0.17 to 17 cm.². In some detail, the authors describe the machine and the way it synthesizes, using normative formant data from independent studies; also, they discuss the intelligibility of the vowels thus produced and the implications of speech synthesis for linguistic research. Another synthesizer, described by Harris (56), produces unnatural but intelligible speech; not just vowels. Elementary speech units or "modules" are stored on magnetic bands, these having been derived from Harris' first study (55) mentioned above. An ingenious method for preserving certain time relations in continuous speech is included in this device.

Message interference.—The many situations in which listeners must identify and attend to one among several competing message sources have given rise to several related studies. Poulton (108) asked observers to attend to one of four sources of airplane-control-tower language, the one being identified by a call-sign different from the others. The observer was to indicate the air-

craft number making the call, the time, and the one of two loudspeakers from which the call came. Better performance was obtained when the two loudspeakers were separated horizontally by 180° than vertically by only 6° . Instructions, telling the observer which speaker to listen to and when to listen, improved the scores still further. Two kinds of errors, omissions, and mishearings, were distributed independently. It appears that a message is likely to be omitted when competing messages surround it in time (that is, just precede or succeed it). Mishearings, in which the wrong message is heard, more often occur when competing messages arrive at the same time, giving an effect like masking. In general, omissions are related to factors that contribute to inattention, while mishearings result more from physical interference. The effect of spatial separation of competing sources is further demonstrated in a study by Broadbent (18), who had observers respond to auditory questions about a visual display. The observer was to respond, however, only to those questions that were preceded by a particular call-sign. Any kind of spatial separation between sources, whether real or synthetic, whether in outside space or between the ears by way of earphones, improved the performance. The positive effects of spatial separation deteriorate, however, if the observer is forced to alternate his attention between the two sources more rapidly than one or two times per second. In the same paper, Broadbent reports that when, in a digit-memory-span test, half of the numbers are presented to one ear in alternation with the other half to the other ear, the observer writes down all the numbers heard in one ear before writing the first number heard in the other. Thus a temporal translation of the spatial separation is observed in the responses.

A less quantitative study of similar phenomena is reported by Cherry (22), who observes that two simultaneous messages spoken by the same talker are very difficult to separate when recorded on the same tape. Putting one message in each ear, however, improves the understanding of both. This effect is even more dramatic for messages rich in clichés and other slogan-type phrases that do not maintain meaningful internal relations over long periods of time. Alternating the messages between the ears, one to the right and the other to the left and then vice versa, yields good intelligibility for very low and high rates but not for intermediate ones.

OTHER PSYCHOPHYSICAL STUDIES

Relations between vision and audition.—As was pointed out above, both Hudgins (70) and Leonardelli (85) have demonstrated a superiority in hard-of-hearing children when eyes aid the ears cooperatively, and vice versa. Sumby & Pollack (125) have demonstrated a similar result in more quantitative fashion. They presented different sized vocabularies of spondee words to listeners in the presence of noise. Half of the listeners faced the talker while the other half faced away. Scores were higher for audition and vision together than for audition alone, the difference being greatest for small vocabularies and low signal-to-noise ratios. Mowbray (94) presents somewhat less definite conclusions from a study in which an experimental group heard and

saw passages of continuous text of varying degrees of difficulty. The visual exposure, not to the talker but to the printed version, was abnormal in that it was paced, line by line, approximately with the auditory presentation. No clear differences between this group and the control group that heard and saw the passages separately emerge, probably because of the difficulty encountered in dividing the attention between the auditory and visual presentations. A quasi-theoretical paper by Mikus (91) points up the difficulty of certain discriminations in language when only audition is used; he further outlines the nature of "phono-visual" effects by which perception is made more complete and definite through the cooperation of the two senses.

Dijkhuis (29) reports an interesting experiment in which observers first saw the experimenter trace out various letters, numbers, geometrical forms, and familiar objects with a stylus on a rough surface. Certain letters and forms were then recognized quite accurately when only the sound of the stylus was perceived.

An additional experimental finding has to do with the influence of sound on the visual critical flicker frequency. Grignolo, Boles-Carenini & Cerri (53) report that a 2000 c.p.s., 85-db tone presented to either one ear or both ears increases the cff.³ for light in the ipsilateral eye, but no change occurs when sound and light are presented to ear and eye on opposite sides. Binocular cff. is unaffected by acoustic stimulation. Tones at much higher or much lower intensities do not modify the cff.

Finally, Myklebust & Bratten (96) have contributed information to this area by reporting on the visual perceptions of deaf children. In a variety of perceptual and perceptual-motor tasks, a group of deaf children was markedly inferior to a normal control group. There seems to be no evidence in this study for better visual perception in deaf children, so often suggested as compensatory for defective hearing. There remain other visual dimensions to be tested before this conclusion is established generally.

Loudness.—Garner (47) has proposed a modified technique for deriving a loudness scale that involves two judgmental procedures: equisection and fractionation. He avoids judgments of equal ratio, primarily because the listener, though judging equal ratios, may not know what number the judged ratio actually has. Garner finds that individual loudness scales are more nearly like each other under these procedures and, further, that better agreement is found between these scales and classical one-ear versus two-ears data. Robinson (110) feels that Garner's procedures are too complicated and unnecessary. He claims that listeners can judge numbered ratios consistently. Using a loudspeaker presentation of a 1000 c.p.s. tone, white noise, and two reproductions of jet noise, Robinson reports close agreement between twice-loudness and ten-times-loudness judgments. At low levels some discrepancies appear between half-loudness and twice-loudness judgments. A single loudness function appears adequate for all observers, represented by the formula, $\log S = 0.029 (P + 40)$, where S is loudness in sones and P is loudness level in phons. Some validity for the standard methods for calculating the loudness of noise is demonstrated by Callaway & Hall (20) who asked listeners to place

about 100 samples of truck noise into six loudness categories. A correlation of .94 was obtained between these judgments and calculated loudness. Lower correlations were found between these judgments and sound pressure level or level according to the A-scale of a standard sound-level meter.

DL for frequency.—A warning has been issued by Rosenblith & Stevens (114) against using extant data on the frequency DL for theorizing about the channel capacity of the ear. They show striking measured differences in the DL depending on psychophysical procedure. When a judgment between the two frequencies is required to be "same" or "different" the DL is lower than it is under the ABX procedure, in which the listener judges whether the third tone of a series is more like the first or the second.

Auditory fatigue and adaptation.—Harris (59) has described a technique in which observers tap while they hear a tone, after a stimulating tone has been turned off. Thus, he can study recovery by gradually reducing the intensity of the test tone so long as the observer keeps tapping. Using this technique on 66 observers, Harris studied recovery curves for 4096 c.p.s. after stimulation by 2048 c.p.s. When recovery time is plotted against sensation level, the recovery curves are more alike than when recovery is plotted as a function of the sound pressure level of stimulation. In another study, Harris (58) reports that fatigue (in db of hearing loss) is a linear function of duration with intensity as parameter, and this function becomes less linear as intensity increases. In this study the fatiguing tone was 750 c.p.s., at levels from 120 to 140 db for durations from 30 sec. to 10 min. Fatigue for 1000 c.p.s. was measured.

Harris & Rawnsley (60) distinguish adaptation from fatigue on four bases: first, fatigue increases with duration of stimulating tone from 30 sec. up to 10 min. while in adaptation duration does not cumulate its effects between 0.05 and 10 sec.; second, recovery from adaptation is linear with time, while recovery from fatigue shows negative acceleration; third, adaptation produces maximum effect at the stimulating frequency while fatigue is maximum a half-octave above; and fourth, recruitment in fatigue is represented by a straight line on a one-versus-the-other-ear plot, while in adaptation it is represented by a curve. The authors attribute adaptation to the end-organ because of the behavior of the recruitment phenomenon under adaptation (see above). In studying adaptation effects in the beat-frequency regions around 100, 1000, and 4000 c.p.s., Harris & Rawnsley (61) find more peaking than is found in the pattern of travelling-wave distribution in the cochlea, but less peaking than in the data on the frequency DL. They conclude, therefore, that since adaptation occurs in the end-organ, additional peaking must be mediated by central nervous mechanisms.

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SOMESTHESIS AND THE CHEMICAL SENSES¹

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Experimental work in the fields of somesthesia and the chemical senses has been and is still [Adrian (1)] dominated by certain concepts which have proved so convenient that they have come to be accepted without serious question by the majority of physiologists and by many psychologists. These concepts are Müller's so-called "law" of specific nervous energies, and von Frey's extrapolation of this law into the theory of punctate representation of primary sensory modalities. In other words, it is commonly accepted that there are morphologically specific pain, warmth, cold, and touch nerve endings, specific nerve fibres, and specific nerve pathways.

It is for this reason that so much emphasis has been laid on the analysis of somesthesia and the chemical senses in terms of the activity of single nerve fibres, which has resulted in reviews that consist of collations of literature reporting the results of electrophysiological experiments.

Recent anatomical and psycho-physiological work, however, has seriously challenged the validity of Müller's law and von Frey's theory with regard to cutaneous sensibility, and has made it necessary to re-examine all the experimental observations on the sensory nervous system from a wholly different point of view.

This review will, therefore, take a somewhat unconventional form. In the first place, we shall examine and collate the evidence which seeks to refute the accepted concepts and then, in the light of these new observations, examine some of the literature in the field of somesthesia which has appeared in the course of the past year. The literature on the chemical senses was ably reviewed in Volume 5 of the *Annual Review of Psychology* and for this reason has been dealt with less exhaustively on this occasion. The review is intended to be provocative and critical, and necessarily reflects the author's personal opinion of the evidence before him.

SOMESTHESIS

Recent observations on the mechanism of cutaneous sensibility.—The statements which follow are based on a series of publications by Weddell and his co-workers (2 to 21), some of which appeared before 1953. Reference should be made to all of these publications if exhaustive examination of the evidence is desired.

Neurohistological observations have shown that in the skin of the human ear and in hairy skin from elsewhere, i.e., abdomen and forearm, there are but two morphologically separable types of nerve termination: unencapsulated (free) nerve endings and nerves ending in hair follicles of different

¹ The survey of the literature to which this review pertains was completed in June, 1954.

sizes and complexity of structure (i.e., a form of encapsulated nerve ending) [Sinclair, Weddell & Zander (14)]. In the mucous membrane of the lip and in other hairless regions of skin, such as the finger pads, there are likewise only two morphologically separable forms of nerve termination: unencapsulated (free) nerve endings and nerves ending within capsules formed of cells and connective tissue which lie in the dermis (i.e., encapsulated nerve endings). These are very diverse in size, shape, and structure [Hagen *et al.* (15)]. Despite these morphological differences, however, the four commonly recognized (so-called "primary") modalities of cutaneous sensibility can be aroused with ease from all of the sites examined. From this it can only be concluded that thermal sensibility can be aroused from skin that lacks encapsulated nerve endings of the type which previous workers have postulated to be concerned with cold (Krause end-bulbs) and warmth (Ruffini corpuscles). Indeed, unless it is postulated that nerves ending in relation to hairs are concerned with thermal sensibility these observations suggest that thermal sensibility must be related to unencapsulated nerve endings.

Work on the termination of nerves in the skin has been further extended in the course of the present year by the use of new and more refined neuro-histological methods. It has now been shown that all nerves terminate in the skin in the same way; that is, in arborizations of fine, naked axoplasmic filaments which spring from ensheathed parent or stem nerve fibres. The axoplasmic filaments all end freely and, although they may overlap and interdigitate with filaments derived from neighbouring ensheathed stem axons, they never come into actual contact or fuse with them. Some of the stem fibres give rise to filaments which are confined within the layers of hair follicles or within capsules in the dermis where they are doubtless insulated to some extent from transient chemical and thermal changes in their immediate neighbourhood. Other stem fibres terminating in all strata of the skin give rise to arborizations of axoplasmic filaments which terminate freely among the various cellular and fibrous elements of which the skin is composed. For instance, certain stem fibres give rise to axoplasmic filaments which end in relation to the adventitia and media of blood vessels and of sweat glands, others to filaments which end among the cells of the epidermis, and still others to filaments ending among the connective tissue elements of the dermis. None of the filaments terminate on the walls of capillaries although they run and end in very close relationship to them; moreover, the number of axoplasmic filaments seen in the dermis is greater in positions where capillaries are most numerous [Weddell and co-workers (11, 12, 13)].

From this it is clear that neither unencapsulated nor encapsulated nerve endings can be classified on bases of inherent morphological differences but only in relation to the tissues among which, and the distance from the skin surface at which they lie. It must follow then that specific sensory modalities can no longer be considered to be related to nerve endings exhibiting an inherent morphological difference, but to nerves terminating either in relation to different tissues or ending at different distances from the skin surface. Be-

fore accepting this proposition it seemed prudent to re-examine the evidence in favour of the existence of four distinct primary sensory modalities. The work of Lele, Sinclair & Weddell (16) and of Lele, Weddell & Williams (17), was directed towards this end. They have shown that when heat is transferred to or from the surface of hairy skin a whole range of modalities, including cold, warmth, itch, prick, sting, and pain which merge imperceptibly (on a statistical basis) one into the other, can be evoked. These and other observations are most easily explained on the basis that four clear-cut primary sensory cutaneous modalities are convenient descriptive headings rather than actual entities. This, in turn, suggests that sensory modalities are determined by the manner in which discharge of the nerve terminals is caused by different stimuli rather than by stimulation of the specified nerve terminals.

Lele (18) has further clarified the position by showing that, in hairy skin in areas in which only unencapsulated nerve endings are present, contact thermal stimuli are reported as warm or cold only if adequate heat transfer can be shown to have occurred. Thus no thermal sensations were reported when fine quartz needles penetrated the skin to a depth of 4 mm., although touch, prick, itch, tickle, and sting were reported on numerous occasions. On the other hand, it has been shown by Woollard, Weddell & Harpman (19) that steel needles of equivalent diameter elicited reports which included thermal responses, cold being aroused at a more superficial level than warmth. It has thus been concluded that current theories of cutaneous sensibility which postulate specific receptors for each primary modality of sensation can no longer be seriously entertained.

Further evidence of the unacceptability of the conventional view that specific nerve endings subserve specific nerve fibres which, in turn, go up specific pathways to arouse specific sensations is provided by observations on the innervation of hairs in the rabbit ear. This comparison between man and the rabbit must, of course, be accepted with caution but it is probably permissible in view of the close similarity of their pattern of innervation. It has been shown that there are 100,000 hairs in the distal three-quarters of the rabbit ear which are innervated by a total of 5,000 myelinated nerve fibres. Moreover, it has also been shown by the evoked action potential technique that each hair on the back of the ear is supplied by at least two and some by more than six dorsal root nerve fibers. Punctate stimuli thus arouse patterns of action potentials which are, in fact, temporally and spatially unique in the case of each region stimulated [Weddell and coworkers (20, 21)].

Various works containing observations relative to somesthesia will now be reviewed in the light of what has just been said. For the sake of convenience conventional subheadings have been retained.

Mechano-receptors.—During the past year, further interesting and important work, which seeks to formulate a clear idea of the transducing mechanism of sensory receptors, has appeared. Alvarez-Buylla & Ramirez de

Arellano (22) have successfully recorded action potentials from single nerve fibres leaving Pacinian corpuscles in the cat's mesentery. By recording from the nerve at the point at which it emerged from the corpuscle they have shown: (a) that subthreshold mechanical stimuli produce a local negative depolarization which has all the characteristics of the local response described by Hodgkin (66); (b) that local response to two subthreshold stimuli may summate to evoke a propagated action potential; (c) the magnitude of the local response evoked by a subthreshold stimulus is reduced during the refractory period following previous action potentials; (d) "on" and "off" responses are evoked by stimuli 50 μ sec. in duration; and (e) multiple propagated disturbances are evoked by a single stimulus only when the mesentery surrounding the corpuscle is distended or when the corpuscle itself, by any other means, is submitted to a constant pressure.

Using a more refined technique, Gray & Sato (23) have considerably extended our knowledge of the mechanism by which a Pacinian corpuscle transduces stimuli. They have shown that when the Pacinian corpuscle is stimulated three phases of activity can be detected: (a) the receptor potential, (b) the potential arising at the axon terminal within the corpuscle, and (c) the potential which arises at the first node of Ranvier. The potential at the axon terminal can be abolished by an amount of procaine which is insufficient to cause any change in the receptor potential. The receptor potential of Gray and Sato is apparently identical with the local response of Alvarez-Buylla and Ramirez de Arellano, but Gray and Sato have shown that the term "local response" is inappropriate since either the receptor potential or the potential arising at the axon terminal within the corpuscle (which is presumably the start of a propagated impulse) can separately or collectively excite the first node of Ranvier and so set up a propagated impulse. The local response in relation to a nerve fibre (i.e., the subthreshold response evoked by the passage of a small current through the nerve membrane) is intimately associated with the mechanism of the nerve impulse itself and does not occur in the absence of sodium. But the receptor potential is practically unaffected if sodium is nearly absent in the medium surrounding the corpuscle. Although there is no conclusive evidence as to the cause of the receptor potential, it seems to Gray and Sato that the most acceptable hypothesis is that sudden mechanical displacement distorts the membrane (possibly in a specialized area), causing a temporary change in permeability. The fact that either compression or a release of pressure (the "on" and "off" effect) proves to be an adequate stimulus supports the suggestion that the receptor potential is evoked by distortion of the membrane since distortion as such is independent of the direction of the movement.

Weddell *et al.* (12, 13) have drawn attention to the morphological contrast in the skin between all the ensheathed stem nerve fibres and the fine, naked axoplasmic terminals to which they give rise, and have suggested that this may reflect a difference in their physiological behaviour. On the basis, of their work, the receptor potential of Gray would occur as the result of dis-

tortion of the membranes of the naked axoplasmic filaments within the capsule, and the potential at the axon terminal within the capsule would correspond to the passage of the local response to, and arrival at, the ensheathed parent or stem nerve fibre lying within the corpuscle. This hypothesis should also be viewed in the light of the observations of Douglas & Gray (24). These authors have shown that both large and small action potentials pass along cutaneous nerves as the result of a discharge of impulses from sensory nerve terminals in cat's skin when they are exposed to acetylcholine. The discharge can be abolished by blocking agents such as hexamethonium; on the other hand, impulses discharged as the result of mechanical deformation of the skin are unaffected by such agents.

None of these observations is at variance with the idea that the encapsulation of nerve terminals (such as occurs in glabrous skin and in relation to hairs) serves to insulate them from the effect of transient chemical and thermal changes in their immediate environment; on the contrary, it can be argued that failure of acetylcholine blocking agents to affect the discharge of impulses which arise in response to deformation of encapsulated receptors is due to the insulating properties of the capsule. The observation made by Douglas and Gray that mechanical distortion of the cat's skin evokes both large and small action potentials is not incompatible with this idea for, as has been demonstrated on a number of occasions, the axons subserving encapsulated nerves ending in hair follicles may give rise to both large and small action potentials. The significance of this work is considerable for if, as is suggested, nerve terminals can be caused to discharge by distortion of the membrane surrounding them, there is no reason to think that distortion of unencapsulated nerve terminals in the skin may not also evoke propagated action potentials.

Thermal receptors.—Zotterman (25) has recently summarized the observations made by himself and his colleagues on thermal receptors in relation to the relevant literature which had appeared up to the date of his publication. It is a masterly review and leaves no doubt as to the accuracy of his observations or as to his consummate technical skill. Using the evoked action potential method, he has demonstrated the presence of "warm" and "cold" nerve fibres in the tongue of the cat and has listed a number of factors which influence the quantitative aspects of the discharges which he recorded. Lack of space prohibits consideration of all of the relevant details contained in this important work, so an attempt will be made to abstract the most significant observations. Zotterman, leading off from small strands of the intact lingual nerve of the cat, finds that a series of action potentials derived from nerve fibres, whose diameter cannot, on the basis of current theory, have exceeded 3 to $4\ \mu$ in diameter, is always recorded. The application of warmth to the surface of the tongue increases the number of action potentials recorded in a given time from certain nerve fibres, whereas the application of cold reduces or abolishes the activity recorded. Such nerve fibres he calls "warm" fibres. Similarly, he calls "cold" those fibres whose electrical activity is accelerated

by the application of cold and inhibited by warmth. A regular discharge at a constant temperature is recorded from "warm" fibres at temperatures lying between 20° and 47°C. The maximum frequency of the discharge is at the rate of about 3.7 impulses per sec. and occurs between 38° and 43°C. The regular discharge of action potentials at constant temperatures passing along the "cold" nerve fibres occurs between 10° and 41°C. The maximum frequency occurs between 25° and 35°C. and is of the order of 9.8 impulses per sec. Zotterman concluded that the response of "warm" receptors when the tongue is at a constant temperature is aperiodic, whereas there is a periodic discharge of impulses from cold receptors under such conditions. A sudden rise in temperature produces a rapid volley of impulses from the "warm" receptors and sudden decrease in temperature produces a rapid volley of impulses from the "cold" receptors. He also states that there is a "paradoxical" discharge of a phasic character in response to a fall in temperature of more than 8° to 15°C. from "warm" receptors but a "paradoxical steady discharge" in response to constant temperatures between 45° and 50°C.

Although Zotterman states that it is very likely that the central threshold of "conscious cold sensations" is higher than the threshold required to send impulses along "cold" fibres, the decline in frequency of the action potential discharge on either side of that occurring over the optimal "cold" temperature range suggests a basis for the development of an hypothesis that the discharge of receptors is governed by two processes, one excitatory and one inhibitory. Zotterman believes that the excitation which occurs when the temperature is rapidly lowered as well as the postexcitatory depression which follows a change in the opposite direction can be explained only by assuming that there is an interaction of two such antagonistic processes. The findings of Lele *et al.* (17) are in full agreement with the mechanisms which have been shown by Zotterman and his colleagues to be operating, and it is interesting to note that the anatomical studies of Weddell and his colleagues (12, 13) have made it possible to formulate an hypothesis (17) which can satisfy this significant experimental work.

The recent paper by Hensel (26) is of importance for he seems to have demonstrated unequivocally that the phenomena reported by Zotterman as occurring in relation to the cat's tongue also occur in relation to the cat's skin. This demonstration, however, raises certain problems. For instance, Hensel has shown that such phenomena occur only in the glabrous skin of the cat's nose. Thermal phenomena of this type are not demonstrable from the skin of the frog, [Adrian (27)] or from hairy skin in mammals such as the rabbit ear [Lele & Williams (28)].

Hensel states that action potential records from the infraorbital nerve of the cat and dog show that cooling the skin elicits impulses from specific "cold" fibres of an amplitude of from 20 to 50 μ v., which cease when the skin is warmed. Mechanical stimuli give rise to impulses which are three to six times greater in amplitude. Impulses are not recorded from "cold" fibres when the skin is stimulated mechanically nor are impulses aroused in "pres-

sure" fibres by cooling the skin. "Cold" impulses are conducted along the delta group of A fibres (3 to 6 μ in diameter), touch and pressure impulses being conducted along the beta group of A fibres (8 to 15 μ in diameter). The latency of response of cold receptors is between 0.015 and 0.025 sec. Hensel believes that this indicates that the cold receptors lie about 0.1 mm. from the skin surface. At a skin temperature of between 30° and 32°C. the "cold" receptors discharge continuously.

Hensel (29) has also equated his observations on cat's tongue with the phenomenon first noted by Ebbecke in man. If the blood flow to a cooled extremity is interrupted the sensation of cold will be diminished; if after some minutes blood is allowed to return to the limb cold is felt more strongly than ever. During ischemia, says Hensel, most of the cold receptors rapidly cease to discharge but when the blood is allowed to return the frequency rises rapidly and after 15 to 30 sec. reaches its initial value. He thinks that this phenomenon is probably due to the effects of anoxia. However, it would seem that the "thermopile" hypothesis of Lele *et al.* (17) might fit the facts more satisfactorily. Briefly, they have shown that the observed facts concerning warmth and cold sensibility can be explained only on the basis that unencapsulated nerve endings lying in different strata of the skin (some being closer to blood vessels than others) discharge impulses wherever the naked terminals are at different temperatures from that of the stem axon from which they arise. In other words, the skin behaves more like a "thermopile" than a thermometer. The fact that some layers are at a higher temperature than others causes different patterns of action potentials to be discharged when cold or when warm objects are applied to the skin surface.

In an interesting paper Dodt (30) has shown that under physiological conditions both normal and paradoxical sensations of cold and warmth are due to the stimulation of thermal receptors or of the nerve fibres included within them. He asserts that these sensations never result from direct stimulation of their myelinated nerve fibres.

Hensel (31) has also restudied the time factor involved in the excitation of thermal receptors. The impulse frequency from cold receptors is apparently proportional to the rate of change of the temperature. Hensel discusses the theory first put forward by Sand (32) that the impulse frequency of the cold receptor apparatus is determined by the difference between an excitatory process *E* and an inhibitory process *I*.

Finally, it is of considerable interest to refer to a lengthy symposium (33) in which Zotterman expounds his observations and their meaning to an audience of electrophysiologists. This discussion, when viewed in the light of the observations of Weddell and his colleagues, is by far the most important of any on thermal receptors that has yet appeared. The most significant fact emerging from the symposium was that nothing had as yet been established concerning the anatomy of thermal receptors. The statement by Bullock that the pit organ of snakes gives rise to a spontaneous discharge of action potentials which can be accelerated by radiant heat, abolished by cold (within

limits), and caused to discharge by mechanical stimuli, is naturally of particular interest in connection with the demonstration by Lele *et al.* (17, 18) of the existence of what they have termed "the universal end-organ." Both the "universal end-organ" and the pit organ in snakes consist of freely ending, naked, axoplasmic filaments.

Pain.—Because last year's volume did not include a review on the subject of somesthesia, the important book by Hardy, Wolff & Goodell (34) on *Pain Sensations and Reactions* which appeared in 1952 has not as yet been reviewed in this series. This work will therefore be discussed forthwith but in the light of the other papers by these authors which have appeared during 1953 (35, 45). The book, which contains no less than 505 references, deals exhaustively with current conceptions of pain and includes a detailed abridgement of the work of Hardy and his collaborators on the use of radiant heat for the determination of pain thresholds. In the introduction by Boring, and in the first chapter of the book, it appears to be assumed that Müller's "law" of specific nervous energies and von Frey's theory of the punctate representation of primary sensory modalities are substantially correct. It is for this reason that Hardy dismisses the "intensive theory" which holds that pain is the result of excessive (or over-) stimulation of any sensory pathway, be it visual, auditory, thermal, or tactile. He states that such a view is not supported by data now available, for pain is a sensation, not an emotion, and, like other sensations, is mediated by a specific and specialized neural equipment. (He was, of course, correct in making such deductions at the time of writing.) The adequate stimulus for pain sensibility is an alteration in the pain fibre endings by a force of whatever nature at a rate sufficiently rapid that tissue damage results. Within limits, says Hardy, the rate at which the pain fibre endings are altered determines the intensity of the pain. In view of the recent neurohistological observations of Weddell and his colleagues, it can now be seen that there are a number of theoretical objections to the way in which Hardy has designed some of his experiments. For example, in 1937 Hardy, in common with other investigators who believed that the specific receptors for warmth and cold were excited only by conduction of heat to, or the abstraction of heat from, the surface of the skin, was not concerned with the measurement of the temperature of the skin under examination. Indeed, Hardy states that "measurement of skin temperature during irradiation was not made as the thermocouple method is not dependable under such conditions." However, this particular statement is not supported by the work of Whyte (36, 37); also the work of Lele, and Lele *et al.* (17, 18) has shown how important is the measurement of skin temperature in relation to thermal sensibility. Hardy also states that the absence of spatial summation for pain, as determined by his method, provides additional evidence of a physiological kind which is in favour of the notion that the terminals subserving warmth and pain sensations are morphologically dissimilar.

In view of the evidence that there is spatial summation for the pain caused by drawing a pin across the skin [Breig (38)], it again becomes neces-

sary to examine Hardy's experimental design more critically than usual. It is clear that the presence or absence of spatial summation for pain must depend upon the even application of a radiant heat stimulus throughout the area stimulated. Thus, on page 71, Hardy states that with apertures ranging from 0.1 to 4 sq. cm. the intensity of radiation measured in millicalories per sec. per sq. cm. exposed was found to be of the same value to within plus or minus 5 per cent, regardless of the size of the aperture. But on page 93 he gives values for the threshold for pain when exposing areas up to 28 sq. cm. Using a dolorimeter, constructed in a manner similar to that of Hardy, Lele *et al.* (17) have shown that there is a 50 per cent fall-off in the intensity of radiation from the centre to the periphery of a 9.6 sq. cm. area. This was found to be due to the construction of the filament of the lamp used as the source of radiant energy. Thus, despite Hardy's claims, it probably cannot be regarded as proven beyond reasonable doubt that there is no spatial summation for pain.

Again, Hardy states authoritatively that pain is invariably reported when the skin temperature has reached 45°C. His statement, however, is based on experiments in which the initial skin temperature was $34 \pm 2^\circ\text{C}$., and the confirmatory statement by Whyte (37) that the initial skin temperature does not matter is not supported by any figures. However, Williams & Lele (39) have observed that pain can be evoked by radiant heat when the skin temperature lies anywhere between 35° and 46°C. provided the temperature of the skin is sufficiently low at the time of irradiation. Thus, although pain will undoubtedly occur when the temperature of the skin rises above 45°C., intolerable pain is reported at temperatures below 45°C. and when there is no evidence of tissue destruction.

These detailed criticisms are not intended to discredit any of Hardy's work; on the contrary, the methodical and accurate nature of the work done by him and his colleagues has made it possible to accept their statements and, by alterations in experimental design suggested by new anatomical observations, to proceed without having to repeat what has already been established. The clinical application of the radiant heat method for measuring pain thresholds and for determining the relative effects of drugs is a major advance in technique, and the psychological studies which have accompanied its use have proved to be of considerable significance.

Hall (40) has reviewed work on the physiology and psychology of cutaneous pain since 1940 in the hope of formulating a general hypothesis which would allow some integration between the two disciplines. He concludes "The initial general hypothesis would seem justifiably to be that the major causes of variation in pain response are due to central factors—differences in capacity to inhibit response or to perceive the whole stimulus situation in conceptual setting."

Brookhart, Livingston & Haugen (41) have made histological studies of the nerves supplying the pulp of teeth. They have demonstrated that the afferent fibres terminate as small unmyelinated fibres lying in relation to

odontoblasts and without any organized or differentiated structures which might be interpreted as specialized receptors. The parent fibres, however, are myelinated and have a diameter of from 1 to 7 μ with a median in the neighbourhood of 3 μ . The small fibres have conduction and excitation characteristics which resemble those of the A gamma-delta fibres of the cat's saphenous nerve. They observed no slowly conducted impulses such as would have indicated the presence of unmyelinated nerve fibres.

Skouby (42) has shown that acetylcholine, acetyl-beta-methyl choline, decamethonium, and succinylcholine sensitize the pain receptors in human skin to subthreshold physical (heat) and chemical (histamine) stimuli. The application of histamine, potassium, or both in subthreshold amounts did not influence the pain threshold to heat. The sensitizing effect of the cholinergic substances was counteracted quantitatively by atropine, curarine, and flaxedil.

Hardy (35) has studied the pain threshold and the flexor reflex threshold in man, the reflex threshold for contraction of the cutaneous maximus muscle in the guinea pig, and the reflex threshold for the tail flick in the rat. In all four cases a skin temperature of 45°C. was found to evoke the pain and reflex responses, respectively. The time during which the skin temperature was elevated had no effect on the response. Hardy concludes from this that the production of pain is due to the thermal inactivation of tissue proteins, probably of the pain fibre endings themselves.

The effect of heat in the production of cutaneous pain and "flare" was studied by Benjamin (43). He measured reaction times, surface skin temperatures, and subcutaneous temperatures at different levels of heat exchange. In general, the thresholds for pain and "flare" were very close, but at a low level of heat exchange the threshold of pain was the higher and at a high level of exchange the threshold for "flare" was the higher. Similar observations followed the application of heat by conduction or radiation. These observations appear to confirm those of Weddell & Pallie (8) that unencapsulated nerve endings in skin may function independently of the ensheathed stem axons from which they arise. With these observations the latter accounted for the "nocifensor system" (Lewis) of cutaneous nerves.

Using electrical stimulation to determine cutaneous pain thresholds, Müller, Loeffel & Mead (44), concluded that prick, the only clear-cut pain end point, cannot be evoked when skin impedance is low. They suggest that prick is not an index of pain threshold but occurs during the breakdown of skin impedance and is, therefore, dependent on the di-electric properties of skin.

Mechan, Stoll & Hardy (45) employed the thermal radiation method to compare the pain thresholds in Indian, Eskimo, and white subjects. They found no significant differences.

A new method for measuring the intensity of pain has been proposed by Swartz (46); it involves using a 60 cycle A.C. current to stimulate the tooth pulp. He concludes that within the range of intensities that were employed

there was a one-to-one relationship between the intensity of the stimulus and the estimation of its painfulness. The value of this technique is obviously limited.

Skouby (47) has shown that subthreshold quantities of pain-producing chemical stimuli (histamine), when injected intracutaneously together with acetylcholine, can produce pain unaccompanied by itching.

An interesting study on the sensations aroused in blistered skin after being rendered ischemic has just been reported by Nathan (48). When a limb on which small blisters had been produced experimentally (but from which no spontaneous pain sensations were reported) was rendered ischemic, the blistered areas became painful. Nathan concluded that this effect was due to an increase in the irritability and thus discharge from the free nerve terminals which had been spontaneously discharging but at a subliminal level before the site was rendered ischemic.

Landau & Bishop (49) have studied the pain induced in human subjects by stimulation of various nerve endings in skin and subdermal tissues by electrical and mechanical means. Differential nerve block was induced in some experiments, the C fibres being blocked by procaine and the delta fibres by pressure. They assert that C-fibre activation, as compared with delta-fibre activation, evokes a longer latency and a penetrating and burning ache, rather than a sharp prick or a sting. In addition, they state that summation took place at stimulation frequencies as low as 2 to 5 per sec. as compared with a frequency of 30 per sec. required to arouse a continuous sensation of pricking pain.

In an interesting paper Rodger (50) has reinvestigated the source and nature of the nerves in the cat's cornea. The author's observations agree in substance with those of Zander & Weddell (7) in other animals but his conclusion that the free intraepithelial nerve endings invariably give rise to pain was reached only after a great deal of soul-searching, for he admits that when some of the nerve fibres to the cornea had degenerated the reaction of the animal to stimuli was greatly reduced in certain regions. This reduction in reaction, however, he finds himself unable (presumably as the result of clinical experience) to regard as the result of a change in mode but asserts that both responses were the result of experiences within a single sensory mode.

Peripheral nerves and dorsal roots.—Using the evoked action potential technique, Kuhn (51) has studied the organization of tactile receptors within dermatomes in the cat and the monkey. The observations reported in this important paper indicate that each dorsal root nerve filament subserves only a limited zone within each dermatome. The cutaneous fields pertaining to each filament are so arranged that they overlap one another in serial order. When a few hairs in the centre of each area were stimulated by brushing, large potentials were evoked whereas towards the periphery of each innervated area brushing evoked potentials which gradually diminished in amplitude. It is also noteworthy that, while certain dorsal root nerve filaments contained chiefly afferents subserving touch endings, others appeared to be

composed solely of nerve fibres coming from muscles and joints, for the intensity of the outbursts elicited by brushing hairs usually varied inversely with that obtained by manipulation of the limb.

Spinal pathways.—In an impressive paper, Brodal & Kaada (52) confirmed (by the evoked action potential technique) the presence of ascending fibres in the pyramid of the cat which they had previously demonstrated by anatomical methods. The fibres arise in the skin, in muscles, and in joints. The authors estimated that between 70 and 80 per cent of the ascending exteroceptive impulses in the pyramids travel along fibres which do not relay before reaching the cortex; the remaining 20 to 30 per cent relay in the nuclei of the dorsal funiculi. On the other hand, about half of the proprioceptive impulses pass along fibres which relay before reaching the cortex. They further suggest that these ascending fibres may play an important part in the coordination of differentiated and skilled movements and that it may be that this is the pathway which subserves the afferent arc of plantar and abdominal reflexes and of placing and hopping reactions.

Gardner & Haddad (53) have shown that, when peripheral nerves in the cat are stimulated, evoked potentials can be recorded bilaterally from the somatic cortical areas 1 and 2. It appears that impulses from all the nerves stimulated can reach the cerebral cortex by way of the dorsal funiculi, but there are, in addition, crossed and uncrossed pathways on each side of the remaining part of the spinal cord. It appears that any of the nerves can project to any of the somatic areas by any of these pathways. The afferent fibres all relay in the lower part of the brain stem.

The action potentials evoked in the midbrain of the cat by stimulating peripheral nerves have been investigated by Morin (54). Nerves carrying impulses from skin and from muscles and joints of both hind legs project on to the tegmentum of the midbrain in both lateral and medial zones. The results of ablation experiments indicate that impulses from both sources can reach the midbrain via a number of pathways lying in the ventrolateral funiculi of the spinal cord.

Gardner & Morin (55) have recently shown that electrical stimulation of either cutaneous or muscular nerves in the hind legs evokes action potentials bilaterally in the pre- and postcentral gyri of the monkey, as was previously demonstrated by Gardner & Haddad (53) in the cat. Evoked potentials were obtained from both gyri after section of the dorsal funiculi at cervical, as well as thoracic, levels. Evoked action potentials were also obtained after incisions which left intact only one lateral and one ventral spinal funiculus. This shows that impulses from either hind leg can reach both cerebral hemispheres by way of one ventrolateral route in the cord. Their conclusion that "impulses of different modalities can ascend in the ventrolateral region of the cord" seems, however, hardly warrantable (although it may be correct) on the evidence they offer, for maximal electrical stimulation of a cutaneous nerve presumably activates all the nerve fibres present and hence the fact that some of the fibres travel in the ventrolateral region of the cord cannot

necessarily be interpreted as meaning that different modalities are contained therein. These findings are in contrast to those of Ruch, Patton & Amassian (56) and must, to this extent, be viewed with caution.

Brain stem pathways.—French, Verzeano & Magoun (57) have noted the differences in form of the evoked afferent discharges in the medial and lateral parts of the brain stem in the monkey. The action potentials in the classical lateral sensory pathway were rapidly conducted and modality segregation occurred; in addition they were projected discretely to the primary receiving areas of the cortex. The action potentials in the medial pathway, on the other hand, were more closely conducted and included all modalities; moreover, they were distributed to extensive areas of the cortex by way of diffuse thalamic projection systems. Upon stimulation of a sensory peripheral nerve, a volley of afferent impulses is conducted to the cortex simultaneously by both systems. The lateral system appears to subserve perception and discriminative sensory functions, while the medial system arouses consciousness or increases alertness. Both functions are, of course, mutually interdependent.

The same authors have made a further study (58) of the effects of anaesthesia on these two afferent conducting systems. Impulses passing along the medial system were blocked by the administration of ether or pentobarbital-sodium while the number of impulses passing along the lateral system and reaching the sensory cortex was undiminished, perhaps even increased, although later cortical events in the complex evoked potentials were altered in character. These findings suggest that the medial brain stem sensory conducting system is composed of multisynaptic relays which make it more susceptible to anaesthetic block than the lateral pathways. It also appears that interneuronal relay systems in the cortex are influenced by the anaesthetic agents employed. They thus conclude that the depression of activity which was noted contributes to a considerable degree to the production of an anaesthetic state. Both these papers are impressive and contain new and interesting observations.

In perhaps the most important paper in this field, Adey, Carter & Porter (59) have shown that there is strong evidence that deep somatic sensibility is represented bilaterally in the first somatic area in both the phalanger and the rabbit. The homolateral representation of each limb occupies an area concentric with and, in most cases, only slightly smaller than the contralateral representation. The afferent volleys from homolateral and contralateral limbs show constant patterns of temporal dispersion over the cortical surface and these are clearly different for the two limbs. The evoked potentials from the homolateral limb show an essentially constant latency across the responsive area whereas the latency of contralateral responses decreases rapidly from the periphery to the centre of the area. In the stimulation of the prehensile forelimb of the phalanger the total responsive zone in the contralateral first somatic area remains constant in size and disposition from stimulation of widely separated points in the digits and distal forearm. However,

the cortical point within this area yielding the most rapid responses shows constant alterations in position with changing sites of peripheral stimulation and it is suggested that perception and localization may occur in terms of the disposition of the point of shortest latency within an activated area of essentially constant site and dimensions. The importance of this work cannot be overestimated. It is as significant in relation to the field of sensory perception as were Walshe's criticisms of the classical theory of punctate representation of voluntary motor activity in the motor cortex.

Amassian has reported (60) that association area positive responses in the anterior portion of the lateral gyrus of the cat are partly dependent upon afferent volleys being relayed from the sensory motor cortex. They are apparently direct and indirect relays from areas 1 and 2. Single unit analysis revealed that the same cell may be fired by stimulation of all four limbs as well as by auditory stimuli but that the initial latency, interspike intervals, and number of discharges of the unit may differ with the site of peripheral stimulation. He concludes that "differences in temporal patterns of association cell discharge may aid in representation of space in the periphery."

Gellhorn, Koella & Ballin (61) have shown that in lightly anaesthetized cats nociceptive stimuli increase the responsiveness of the auditory projection areas to acoustic and the visual, to optic stimuli. This increased activity is thought to be attributable to the interaction at a cortical level of impulses evoked by nociception and those originating in the specific projection systems. This confirms Bernhaut's findings that afferent impulses may influence cerebral cortex potentials in two different ways: they may induce either a diffuse excitation (commonly obtained by the application of nociceptive stimuli) or cause an excitation which is confined to a specific projection area (regularly shown with acoustic or optic stimuli).

It is clear from an analysis of this literature on somesthesia (culled from widely differing fields of experimental work) that sensory experience is now being considered in terms of a spatio-temporal pattern of nervous activity rather than a series of discrete connections within a limited number of modes.

It appears that none of the observations which have been made recently are at variance with this new conception. In fact, it is now difficult to interpret some of the observations in the light of the more conventional view which has dominated this field for so long.

CHEMICAL SENSES

Adrian's work (62) continues to dominate electrophysiological research in this field. The problems of olfactory discrimination are more like those of the ear than the eye in that the stimulus has no spatial attributes which must be signalled independently. Thus, the spatial distribution of the excitation could give information as to quality of smell as it does with quality of sound. However, there are differences in the spatial distribution of the excitation with different smells and also differences in the time course of the discharge and in the particular receptors which are excited at any point. Of these dif-

ferences, the only one which would serve to distinguish more than a few general types of molecules is the difference in sensitivity of the individual receptors. However, the spatial and temporal patterns are important. The size of the olfactory epithelium in macrosmatic animals is very large and numbers of individual receptor cells in those animals are more than the number of retinal receptors. Adrian has not been able to lead off the nerves entering the ethmoid plate because they are unmyelinated, his records being obtained from the mitral cell layer of the olfactory bulb. Water-soluble substances have a lower threshold in the anterior region (of the rabbit) whereas with lipid-soluble substances the threshold is lower in the aboral region. In the middle of the bulb, however, considerable differences are found in the sensitivity of the individual receptors which lead to a single mitral cell. Apparently specific units exist for many different substances. When leading off from one region, large and small potentials are detected when a substance is allowed to stimulate the olfactory epithelium. Adrian assumes that the series of large spike units are from the discharge of one mitral cell, while the small ones come from neighbouring units leading from other groups of receptors. Thus, it is nearly always possible to find one substance which, in low concentration, will give only the large spike discharge, all other substances giving the smaller spikes or a mixture of large and small spikes. Each large spike seems to have a special relation to a particular stimulus: e.g., acetone gives a discharge consisting of large spikes even at high concentrations and pentane gives only small spikes; and amyl acetate stimulates the large spike unit but stimulates a number of others as well, whereas pentane does not affect the unit which reacts to acetone.

Of considerable interest is the observation that the physical and chemical properties of the substance will determine the time course of excitation. With a particular region, for example, the large spike unit has a specific sensitivity to xylol. As the concentration of xylol in the air is increased, other units will begin to come in during the later part of the discharge. With pyridine and eucalyptus the smaller spikes appear first and the large ones come in later on. The result is that the photographic reproductions of the discharge have a characteristic shape for each substance. Although Adrian hesitates to conclude that the brain identifies the smell by the same criteria, he states that it would be easy on this basis to see how a great variety of smells might be distinguished without the need for very great variations in the receptors. If this is so, Adrian feels that it would be possible to relate olfactory discrimination both to auditory and to visual discrimination. Tones are distinguished by the general regions of the basilar membrane which is excited; visual scenes, by the detailed pattern. Smells seem to be distinguished by a combination of detailed pattern and general region. But when all this is clear the central problem of the nervous system will still be to explain how the spatio-temporal patterns of incoming discharges are dealt with by the brain. This is a most impressive and thoughtful paper.

Using an evoked cortical action potential method, Ward (63) has shown

that stimulation of the lateral olfactory tract activates the anterior and medial aspects of the pyriform area. This involves the regions of the cortical amygdaloid nucleus, the nucleus of the lateral olfactory tract, and possibly the central and a part of the basal amygdaloid nuclei. The lateral amygdaloid nuclei do not seem to be involved.

GUSTATION

Bagshaw & Pribram (64) have studied the acceptance threshold of quinine, before and after experimental cortical ablations, in immature monkeys. The ablations consisted of bilateral removal of the cortex of the anterior part of the insula, the operculum, and the anterior supra-temporal plane. Only complete ablation of all of the three regions led to ageusia. Slight lowering of the acceptance threshold followed ablation of opercular plus insula cortex, but no such lowering followed ablation restricted either to the insula or to the insula plus the anterior supratemporal plane. They compare these results with visual studies in which total incapacity results only if the entire primary visual area is destroyed, and infer the presence of a gustatory "macula," presumably focussed on the junction of the frontal operculum and the anterior insula.

The properties of chemoreceptors in the tongue of the rat have been studied by Beidler (65). When the chorda tympani nerve was dissected from the lingual nerve definite types of evoked action potential response could be obtained. He categorized them into three groups: the first, in response to acids and salts, which rapidly gave rise to a large response; the second, in response to sugars, which was smaller and developed slowly (the response to saccharine was in the first group, not the second); and the third, in response to quinine, ethyl alcohol, strychnine, phenylthiourea, and other such substances, which was barely perceptible. These three groups Beidler compared to the four classical taste modalities with acid and salt combined as one. He felt that the lack of response to bitter compounds was due to the fact that they were mediated through impulses conducted in fibres of the glossopharyngeal nerve. The lingual nerve, if separated from the chorda tympani, was activated only when very concentrated salt or acid solutions were placed on the tongue. Single fibre preparations, dissected from the chorda tympani nerve, responded to both acids and salts. Single fibres responding to salt stimulation also responded to saccharine but not to glucose or sucrose. Occasionally, single fibres responded to the application of sucrose by giving off short arrhythmic bursts of impulses at a slow rate. The anion of a salt appeared to be of much less consequence than the cation in stimulating the "salt" chemoreceptor. The latency of response to salts and acids was of the order of 45 to 55 m. sec., whereas the latency to sugar was so long that no accurate reaction time could be measured. The difficulty in categorizing modalities is thus not confined to cutaneous sensibility.

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INDIVIDUAL DIFFERENCES^{1,2}

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The present lack of integration between concepts developed in the domain of individual differences and current theoretical systems has its roots deep in the history of psychology. Bindra & Scheier (9) in discussing this lack of integration point out that "if the psychometric researcher and the experimentalist agree on anything, and there is some doubt about this, it is that the other kind of psychologist plays in the other league (class B)." The reason for this schism goes back into the history of psychology as a science, for experimental psychologists of the nineteenth century were inclined to regard individual differences merely as sources of experimental error which hindered the establishment of general laws. For this reason the study of individual differences developed in its early days without reference to the expanding body of knowledge derived from laboratory experimentation. The transition of psychology from the study of states of mind to the study of behavior did not at first open a place in systematic psychology for the study of variables commonly described as individual differences, for the early type of behavioristic psychology sought laws of the general type $R=f(S)$ which assumes that responses are a function of the stimuli to which the subject is exposed. When the limitation of such laws became apparent, and a third category of variables referred to as intervening variables was introduced, it became possible for experimental psychology and the psychology of individual differences to become integrated; for intervening variables are those with which the student of individual differences has been primarily concerned with measuring. Nevertheless, as Bindra & Scheier point out, the experimentalist has been mainly interested in intervening variables which can be manipulated through

¹ The survey of the literature pertaining to this review was completed in May, 1954.

² The following abbreviations are used in this chapter: S (subject); TAT (Thematic Apperception Test).

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the control of antecedent conditions, while the psychometric researcher has been concerned with more enduring and usually with more complex characteristics. However, in some recent studies such as those conducted at the State University of Iowa the latter type of variable has been used in an experimental laboratory setting.

Psychometric research and experimental research have also differed in the past in another important respect. Experimental psychologists distinguish between independent and dependent variables and consider events as occurring in a time sequence. Their method is typically that of dependency analysis which seeks to establish functional or causal relationships, while the psychometric researcher's method has been until recently more typically that of correlational analysis in which variables are correlated without concern for the direction in which predictions are to be made and without reference to any functional relationships that may occur among events. These methods differ profoundly, as Spence (62) has shown, in the kind of psychological law which they evolve. Since the writer believes that there is a trend towards basing studies of individual differences on a dependency analysis approach, this chapter is written from that viewpoint in contrast to the corresponding chapter of the previous year which is written mainly from the traditional correlational analysis approach.

The thesis that the study of inter-individual and intra-individual differences can be studied scientifically only by dependency analysis is advanced in vigorous terms in a major work by Stephenson (66). He contrasts dependency analysis methods with what he refers to as interdependency analysis methods which are those correlational methods which seek to establish empirical rather than functional relationships. He states that interdependency analysis, which finds its mathematical roots in Pearson's *Grammar of Science* (52) and which grew and came to bloom in the work of Spearman, Thurstone, Burt, Holzinger, Cattell, and the other factor analysts is a sterile plant which has borne no fruit. He believes that those who have done most to develop interdependency analysis have failed to understand either its implications or the assumptions on which it is based. Stephenson concedes that Burt alone has ever attempted to relate *R*-technique factor analysis to theory of knowledge, but he criticizes him for having linked it to the same nineteenth century metaphysic which became the background for Pearson's correlational approach.

Stephenson presents compelling arguments against interdependency analysis and finds powerful support for his position in the writings of Keynes, Kantor, and Brunswick. He points out that as far back as 1921, Keynes (42) had seriously doubted the validity of inductive inference in correlational theory of the type which Pearson developed. This viewpoint is somewhat similar to that voiced by Bartlett (4) who states that "it seems to me that nothing of any genuine psychological significance can ever be achieved simply on the basis of a knowledge of end results, with no knowledge of at least some of their antecedent conditions."

Stephenson considers that of all the various factor analytic approaches the only one which can be adapted to the scientific study of behavior is that which has become known as *Q*-technique, for this technique, when properly used, performs the functions of dependency analysis. He does not take the position that *Q*-technique can in any way replace the methods of dependency analysis which Fisher has developed, for the latter are the more powerful tools whenever they can be applied, but he believes that *Q*-technique is uniquely adapted to the study of certain problems related to inter-individual and intra-individual differences which are not easily handled by the Fisherian approach.

The application of *Q*-technique as it is developed by Stephenson requires that research be based on a theory concerning the nature of the dependencies to be predicted and their relationship to the independent variable. *Q*-technique is used to test singular propositions related to the theory on which the experiment is based. However, Stephenson tells little about the formal nature of the kind of theory on which he believes studies involving *Q*-technique should be placed. He also leaves the reader at times with the uneasy feeling that the universes that are being sampled are not well identified, but nevertheless the book he has written is an exciting one.

Stephenson also feels that while *R*-technique and interdependency analysis are based on the assumption that all traits are nomothetic, *Q*-technique can be adapted to the study of idiographic traits which have never been successfully handled by those who have worked in the typical psychometric furrow. Many have already voiced a need for studies based on an idiographic approach, and during this last year this need was again presented and defended by Beck (7) and illustrated to a limited extent with data collected on schizophrenic patients. Stephenson's approach may well provide a technique for developing the kind of research which Beck and others have so long advocated. It may be noted that the criticisms of traditional forms of correlational analysis which have just been considered are not essentially different from those which have been frequently expressed by experimental psychologists and which are well summarized by Skinner (61) in his most recent book. Many psychologists would question Skinner's position that a test is necessarily just a sample of the dependent variable for in many cases it is clearly not so. However, particularly worthy of notice is Skinner's reiteration of the point of view that in the typical psychometric approach the prediction is not from cause to effect but from effect to effect and thus does not result in the discovery of functional relationships.

THE DEPENDENT VARIABLE AND THE CRITERION PROBLEM

Difficulties in the selection and measurement of the dependent variable are commonly referred to collectively as the "criterion problem." The selection of the dependent variable to be predicted is necessarily a reasoned judgment; but, unfortunately, this judgment is inextricably bound up with the process of empirical measurement. Until a variable is defined it cannot

be evaluated for appropriateness or relevance, but a variable is not properly defined until it can be empirically measured. There is little discussion evident in the current literature concerning how such reasoned judgments should be made, but there is extensive interest concerning the appropriateness of various procedures for measuring response characteristics that have been selected for measurement. Difficulties stem largely from the fact that it is usual to find that R , a particular response variable which is judged to be a relevant dependent variable, cannot be measured directly. Therefore, indirect procedures must be used for its measurement and questions are raised concerning the logic of these procedures and the validity of the inferences that can be made concerning direct measures of R from indirect measures. The problem is a common one in all scientific work.

The dependent variables in most studies of individual differences pertain to behavior in educational and work situations though they can be derived from other situations. They are variables which describe characteristics of responses to these situations. In the educational field there are substantial possibilities for measuring responses to instruction with objectively scorable instruments, but the responses to the work situation do not usually offer such possibilities. For this reason response variables in work situations have been measured in the past mainly by means of judgments recorded on rating scales. Ratings may be considered as representing a response of subject X to the response of subject Y to situation Z , a most indirect and error-ridden method of measuring the characteristics of the response of subject X .

Vast amounts of data continue to be collected on the relationship of aptitude test scores to ratings and other criteria of job success. An attempt by Brown & Ghiselli (11) to derive generalizations from this mass of empirical fact is noteworthy because it is one of the few of its kind, but the results of their effort indicate the limited extent to which it is possible to derive generalizations from data currently available and stress the need for the development of dependent variables which have properties superior to those at present in use.

Attempts continue to be made to identify variables in this chain of S - R relationships that interfere with the validity of ratings. Schneider & Bayroff (58) studied the relationship between rater characteristics and validity of ratings. Unfortunately, in this study the criterion score against which ratings were validated was the average rating of a number of raters. The findings suggested that the abler individuals, as identified by an intellectual aptitude test, produced more "valid" ratings than the less able. In this study, the forced-choice ratings tended to be less "valid" than the traditional types of rating scales, and they also showed a greater reduction in validity as the ability of raters decreased. This is easily understandable. Bendig (8) explored the problem of the relationship between the number of rating categories and the reliability of ratings. In the case of his data reliability was surprisingly independent of the number of categories used.

Other studies have also used consensus of opinion among raters as the

criterion of validity. A study by Springer (64) using this approach showed agreement with similar previous studies by presenting data demonstrating that supervisors showed more substantial agreement on their ratings of workers than did co-workers. Bayroff, Haggerty & Rundquist (5) conducted a rather elaborate study of relationship of the "validity" of ratings to rating techniques and the characteristics of raters. The data supported the contention that differences in the characteristics of raters are of much greater moment than are differences in the rating techniques employed. Indeed, negligible differences were found between graphic scales and forced-choice techniques, but rater characteristics such as general ability as measured by a test had substantial effects on "validity," the forced-choice technique falling off rapidly in "validity" with low scoring raters. These results are consistent with other studies of the determinants of rater responses.

A much more promising type of approach to the same problem involves the use of an independent measure of the proficiency to be rated. An example of this type of procedure is illustrated by Judy (38) who studied some of the conditions related to the accuracy of judgment of job proficiency. The merit of the Judy study in comparison with those that have already been considered lies in the fact that the proficiency on which the subjects were rated, namely job knowledge, was measured independently by a test. The research demonstrated that, for the population studied, supervisor judgments are less influenced than are those of peers by factors related to acquaintance, marital status, and age and that the evidence tends to indicate that supervisors' ratings should be used as criteria in prediction studies in preference to peer ratings, a conclusion which is contrary to the assumptions on which many recent studies have been based.

A research presented by Klieger & Mosel (43) produced data consistent with the hypothesis that the length of exposure of the rater to the events to be rated is not a factor of central importance in determining the reliability of ratings provided that at least a minimum amount of exposure is permitted.

What was advanced a few years ago as a major attempt to overcome the shortcomings of traditional rating scales, namely forced-choice rating scales, still continues to interest many concerned with problems of prediction. The energies of investigators interested in this technique seem to be devoted mainly to the development of new rating or self-evaluation forms. Runyon & Stromberg (57) developed a form for rating clinical psychology practicum students; Heineman (36) developed a forced-choice form of the Taylor Anxiety Scale; and Taylor *et al.* (67) prepared a forced-choice evaluation form for salesmen. A more novel approach to the same problem of controlling preference values of items is provided by Edwards & Horst (22) who adapted Stephenson's structured sample technique to this purpose. They point out that in many studies involving a *Q*-sort the items that are sorted vary along a dimension of social desirability which may profoundly affect the position assigned to statements and make the results of the analysis of the data almost uninterpretable. In order to overcome this difficulty a Fisherian fac-

torial design is proposed in which social desirability is no longer confounded with the comparisons that it is desired to make.

The proliferation of forced-choice rating forms has done little to extend knowledge of their usefulness and potentialities. Knowledge concerning the latter has been almost entirely the product of scientists in the Personnel Research Branch of the Adjutant General's Office who have diligently pursued a series of studies concerning the relative merits of forced-choice and other forms of rating scale. One of these studies (53) investigated an idea which was put forward some years back that by adding suppressor items to traditional types of rating scales such scales could be given any special advantages which the forced-choice type of scale may have. The findings of this study "in part support this theory," although for the particular length of instrument employed negligible differences were found between forced-choice, traditional ratings in the yes-no format, and the latter combined with suppressor items. When the questionable procedure was adopted of calculating the validity for instruments of infinite length some superiority (of unknown statistical significance) was found for the traditional items plus suppressor format. The results of this carefully-planned study are of some interest, and it is hoped that they will be published in some readily available source as well as in a government publication.

Although ratings are likely to be used for some time to come as dependent variables there are psychologists who are actively searching for criterion variables which are more direct measures of response variables than the "responses to responses" which ratings represent. One alternative procedure is to develop relationships between independent and dependent variables under relatively controlled laboratory conditions.

Although such laboratory situations may lack some of the components of the daily-life situation, it is possible that what they lack in this respect is more than compensated for by the control which can be exercised over the conditions under which observations are made and the objectivity with which these observations can be recorded. They also offer a basis for hope that psychometric research may ultimately develop *S-R* laws rather than the *R-R* laws which have developed in the past and which are of such limited value. An illustration of this newer type of approach is presented by Adams (1) who was concerned with the prediction of performance on a psychomotor task after extensive opportunity had been provided for learning. Specific applications of this type of research are not hard to find. The prediction of proficiency in flying after considerable training is one.

Adams' approach was to train *Ss* on a complex psychomotor task administered under laboratory conditions. Proficiency in this task after training provided the dependent variable of the study. A particularly important finding of this study was that the best predictors of performance on the complex psychomotor task were scores on psychomotor tests after practice had been given on these tests. Adams suggests that this finding can be understood in terms of the hypothesis that the ability to integrate component

skills is measured only by psychomotor tests after sufficient practice has been permitted to provide an integration of component skills.

It should be noted that the purpose of studies such as that of Adams is to develop generalizations concerning individual differences which can be applied to a wide range of situations, and they differ profoundly in this respect from studies conducted by industrial psychologists for developing selection devices for specific occupations. The laboratory approach aims at extending an organized body of scientific knowledge which has wide applicability rather than that of finding empirical solutions to specific problems and solutions which do not add to an already organized body of knowledge.

Many applications of the laboratory approach to the study of individual differences make no attempt to use stimulus situations and dependent variables which resemble those found in work situations and educational situations. A series of studies conducted at the State University of Iowa represent pioneer efforts in this direction. These studies have been concerned with the relation of the scores derived from the Taylor Anxiety Scale to level of drive in conditioning situations, and, more recently, in a new series of experiments by Spence & Farber (63) these studies have been extended to differential conditioning. The results of the Iowa studies are of interest not only for the results they have achieved, but also in the fact that they have opened the way to the study of the interrelationship of test variables within a framework of contemporary psychological theory.

Another example of the study of individual differences from a laboratory and dependency analysis approach is given in a recent volume by Witkin *et al.* (77) which describes an experimental study of the relationship of certain personality variables to performance in selected perceptual tasks. This volume also presents the argument that relationships among variables of the type studied within the domain of individual differences can best be investigated under controlled laboratory conditions. These scientists developed a series of tests in which individuals had to judge the position of an object within a field or the position of the body with respect to the vertical. In the latter test S is seated in a tilted room and required to bring his body into a position which he perceives as upright. Ss showed considerable consistency from task to task in the extent to which their judgments were dependent upon the total perceptual field. It was then postulated that it should be possible to measure the same variable of field dependency by means of personality tests such as the Rorschach and Figure-Drawing, and some evidence was presented to substantiate this point of view.

The methodology of studying individual differences which involves the prediction of the dependent variables under relatively controlled laboratory conditions clearly involves difficulties which, as work progresses, are becoming more and more apparent. The difficulties seem to be at least partly a product of the sensitivity and responsiveness of human subjects to peripheral conditions which cannot be adequately controlled by the experimenter. They are reminiscent of the difficulties which Pavlov experienced

in the early days of his experimental work when he discovered that his dogs acquired a great number of conditioned responses to irrelevant stimuli and that special precautions had to be taken in the design of experimental situations. In the case of human subjects it is much more difficult to control such stimuli, and there may even be some doubts as to whether sufficient control can be exercised in many instances at the present time to permit the reproduction of experiments in different laboratories.

Some of the difficulties of studying test scores as determinants of *S-R* relationships produced under relatively controlled conditions are illustrated in a study by Carlson & Lazarus (14). These researchers attempted to repeat an earlier experiment by Williams (74) who had been successful in predicting from Rorschach scores certain aspects of the individual's behavior under stress. In the Carlson and Lazarus experiment stress was produced by electric shock, failure information, and the presence of several observers. The task performed under threat was the Digit Symbol test from the Wechsler-Bellevue. No relationship was found between Rorschach variables and decrement in performance under stress which is the direct opposite of the finding in the Williams experiment.

This inconsistency in experimental results is what one may expect to find in these early explorations with complex dependent variables produced under laboratory conditions. Until some more satisfactory explanation is offered, lack of reproducibility of results must be attributed to uncontrolled and unidentified antecedent or concomitant conditions which affect responses to the situation in which behavior is to be predicted. Control over significant variables related to antecedent conditions can be obtained in some instances by exercising control over the selection of subjects.

These difficulties pertaining to the development of dependent variables are also inherent in many studies related to the development of tests from which independent variables are to be derived. Since these difficulties are receiving particular attention in the domain of projective techniques, it is desirable at this point to undertake a review of what has been learned in that connection.

PROBLEMS OF REPRODUCING *S-R* RELATIONSHIPS IN THE DEVELOPMENT OF INDEPENDENT VARIABLES INVOLVING COMPLEX PROCESSES

Responsiveness of *Ss* to irrelevant stimuli does not seem to have been a factor which seriously disturbed early attempts to measure individual differences. However, in more recent developments and particularly those related to projective tests it has become more and more apparent that such responsiveness probably accounts for much of the inconsistency among studies.

The year added to its crop of inconsistencies in this area. For example, Rubin & Lonstein (56) attempted to repeat an earlier study by Thiesen (68) which identified certain Rorschach variables as being associated with schizophrenia, but could find no evidence to support Thiesen's claim. It is impor-

tant that knowledge be obtained concerning the reasons for such typical inconsistencies. For this reason a paper by Miller (51) on the prediction of behavior by means of the Rorschach assumes particular significance since it is an attempt to identify and organize the variables which determine the response of the subject to a projective device. He points out that attempts by clinicians to validate the Rorschach have been largely directed towards the discovery of empirical relationship between signs and symptoms, and there has been little or no attempt to relate these discoveries to any identifiable theory of behavior. Miller points out that a Rorschach protocol is a product of at least five categories of variables. These are (a) the circumstances under which the individual is tested, whether he is tested voluntarily or involuntarily, whether he wishes to produce a "good" or a "poor" record, etc., (b) the way in which the task is introduced, (c) the characteristics of the examiner's behavior, (d) the subject's personality structure, and (e) other relationships of the examiner and the subject. Considerable empirical evidence exists that all of these classes of variables are important determinants of the subject's response. This leads to the inevitable conclusion that if understanding is to be achieved concerning the predictive value of Rorschach protocols and scores, those who plan future studies must control many variables which have not been controlled in the past. Only when the test is administered under reproducible conditions can one expect to achieve reproducible experiments and consistent "validity coefficients." Related to Miller's review is an article by Hammond (35) which points out that in most research in clinical psychology the design should permit the experimenter to estimate the variance attributable to differences in test administrators.

Much still needs to be learned about responsiveness to irrelevant stimuli in the administration of projective devices, and in this respect an article by Gibby *et al.* (30) is particularly relevant. These scientists conducted an experiment in which 12 Rorschach examiners, who used the same system for Rorschach administration, tested at least 20 cases each assigned to them at random in a Detroit clinic. In the selection of examinees an attempt was made to obtain divergent personalities. The results of this study demonstrate clearly the role of the examiner as a source of variation in Rorschach scores and are presented by the researchers as a foundation for further studies which, it may be presumed, will relate specific examiner variables to responses to the Rorschach.

The kind of cautions which Miller urges are beginning to have influence on the interpretations given to research data derived from the administration of the Rorschach. For example, Walters (72) administered the Rorschach to 50 prison inmates and found the records were similar to those of preadolescent boys, but with admirable caution Walters goes on to state the results may be a product of the situation under which testing occurred. Such caution in the interpretation of Rorschach protocols would have been unheard of only a few years ago.

Difficulties involved in the replication of the results of studies that in-

volve Rorschach variables as intervening variables are paralleled by similar difficulties in other relatively new areas of psychological measurement. Attempts to account for inconsistent results when experiments are repeated usually involve the introduction of other variables. An example of this is a study by Brown (12) of the relationship which exists between measures of rigidity and measures of authoritarianism. Brown had repeated some of the earlier studies in which such a relationship was found, but he found no relationship at all. He then hypothesized on the basis of what was known about previous experiments that his results differed because Ss were not tested under conditions involving high ego involvement. In a new experiment he was able to demonstrate a significantly positive correlation between measures of rigidity and measures of authoritarianism in an ego involved group, while in a non-ego involved group the correlation was zero.

One approach to the improvement of measures derived from projective devices is to identify irrelevant determinants of scores and then to exercise control over these determinants. Another approach is to modify the stimulus in such a way that it becomes a central determinant of the response. This seems to have been the approach of McClelland *et al.* (48) in their studies of achievement need which have now been brought together and published in a single volume. One suspects that responses to the TAT²-type of instrument are less subject to the influence of irrelevant aspects of testing conditions than are responses to the less structured Rorschach. Lack of structure seems to bring with it responsiveness to irrelevant stimuli.

What has been said here should not be taken to imply that the year has not produced studies pertaining to the "validity" and reliability of specific scores derived from particular projective devices, for it has. Lindzey & Goldberg (44) undertook an experiment to determine whether hypothesized differences in motivation of men and women were reflected in corresponding differences in motives indicated by responses to TAT words. The findings were consistent with the hypothesis of the greater sexual needs of men and the greater abasement, nurturance, and verbal repressiveness of women. Differences between men and women were not significant at the 5 per cent level in achievement need, aggression need, and narcissism. Wirt & McReynolds (76) studied the reliability of the Rorschach variable R by a split-half technique which minimizes administrator and other irrelevant determinants of response. Under these conditions the reliability coefficients, corrected by the Spearman-Brown formula, for three groups described as "normals," "neurotics," and "schizophrenics," were .88, .91, and .77 respectively. Differences in these three coefficients are probably mainly a product of differences in range of scores.

Other studies pertain to specific uses of the Rorschach. Wertheimer (73) was unable to find a relationship between paranoid disposition and "eye" responses. Graver (32) collected data consistent with the hypothesis that paranoid schizophrenics who showed anxiety on the Rorschach are more amenable to treatment than those who do not. Bandura (3) studied the rela-

tionship of white-space responses on the Rorschach to oppositional behavior. It is commonly assumed that the inferences to be made from such responses must depend upon whether they occur in an introverted or extroverted personality. Bandura found a relationship between number of white-space responses and ratings for negativism, but this was independent of personality type. Related to this finding is the conclusion of Ingram (37) that high frequency of white-space responses was related to aggressiveness of response in a frustrating situation.

Keehn (39, 41) made careful and critical reviews of studies pertaining to color as a determinant of responses to the Rorschach and could find no convincing evidence to show that responses to color could be used to diagnose neuroticism. This conclusion accords well with the results of Brody (10) who found little difference between "neurotics" and "normals" in color shock as measured by reaction time. In a further related study, Keehn (40) investigated the relationship between scores from tests involving a color response and scores hypothesized to involve a similar response in the Rorschach. A factor analysis yielded a color-response factor, but the Rorschach scores did not load on this factor but on a second factor which did not appear to depend on color reaction.

THE IDENTIFICATION AND MEASUREMENT OF NEW INDEPENDENT VARIABLES

Butler (13) has pointed out that mental test theory consists of a set of postulates and theorems which apply to tests regardless of their content. These theorems and postulates pertain, not to behavior, but to theory of measurement. The result has been that psychometric tests usually satisfy certain conditions highly valued by measurement theorists, but they do not satisfy any identifiable set of psychological principles. This is particularly noticeable in the case of personality inventories, the content of which is usually derived, not from a formal psychological model, but from whims and hunches. There are, of course, a few exceptions, such as introversion-extroversion scales, which mostly have a rather remote relationship to Jung's psychological theory.

The model which Butler proposes to use in the building of personality inventories is that developed by Tolman (69) in his more recent writings. This model is used by Butler to establish certain hypotheses concerning the characteristics of inventory items which are most likely to provide useful intervening or independent variables. Personality inventory items which are most promising for predicting behavior should, according to Tolman's theory, refer to behavior space, feeling states, and the belief-value matrix. Least valuable should be those that refer directly to the needs system since the postulated relationship between the needs system and behavior space is indirect and through the belief-value matrix.

Butler suggests that *Q*-methodology may be well adapted to the study of the dimensions along which the belief-value matrix is generalized, but he has

not, as yet, worked out in detail any examples to show how this may be done. However, his suggestions are thought provoking, and he should receive widespread support for his statement that much can be gained by building tests in accordance with a psychological theory as well as in accordance with the postulates and the theorems of psychometrics.

Perhaps more advanced in the development of measures of personality in terms of a psychological theory is the work of McClelland (48) and his associates. The type of theory which this group has adopted is much simpler in form than the more recent theoretical developments of Tolman. The essential core of the theory is that cues become associated with affect, or changes in affect. Later, when these cues are presented the affect, or change in affect, becomes reintegrated. High and low motivational conditions should be producible by exposing the subjects to cues which reintegrate affective states or by withholding these cues. McClelland and his associates demonstrate that when cues are provided which are hypothesized to activate the achievement motive that subjects show an increased productivity of "achievement imagery" in responding to a TAT-type of test. Further studies showed that the *n*-Achievement scores were demonstrated to be related to the productivity of subjects in such tasks as simple arithmetical computation and the amount of improvement shown in the ability to do anagrams. Achievement need scores were also related to college grades, but they did not correlate with ratings of motivation. The writer believes that the latter is completely in accordance with expectation, for a person who is attempting to rate motivation will tend to avoid rating merely achievement, but will rate for some characteristic which, according to some private theory of his own, is symptomatic of motivation. It seems highly unlikely that such ratings, based on each rater's own theory of what is symptomatic of motivation, will correlate with measures of motivation.

The studies of Butler and McClelland *et al.*, although they are given emphasis here, are not typical of those directed towards the development of new instruments. Most studies designed for the latter purpose still seem to be based on the assumption that fundamental variables can be "discovered" by the application of mathematical techniques to data derived from instruments which have been assembled without the help of a formal psychological model. It is this type of study which must be considered now.

Several major enterprises in the application of *R*-technique factor analysis appeared during the year. The reviewer is left with the impression that there is a decline in the number of such studies but most of those that are published represent mainly the results of major and costly enterprises. Perhaps this reflects the viewpoint expressed by Eysenck (25) that if factor analytic studies are to have value, then they must be conducted on a grand scale.

An excellent review of the measurement of psychomotor abilities was prepared by Fleishman (26). This review describes the historical development of the field, problems in the building and administration of such tests, and

the predictions which can be validly made from them. Fleishman believes that seven factors in the domain have been, as the saying goes, well identified and that three "somewhat less well-defined" dimensions seem to exist. The seven factors which Fleishman considers to be well identified can be described briefly as reaction time, tapping, manual dexterity, finger dexterity, steadiness, aiming, and gross precision of movement.

Fleishman's review and summary of factor studies in a particular area is one of the few which has been followed up by an attempt to build tests which define operationally the factors which have been postulated. The other factor analytic studies reviewed end with dictionary-type definitions. Fleishman in a subsequent study (27) assembled a battery of 38 psychomotor tests which provided at least three tests of each one of the major factors described in his review and administered the battery to 400 subjects. The resulting matrix of correlation coefficients was factored by a technician who had no knowledge of the nature of the tests. Orthogonal rotations were continued until simple structure and positive manifold were approximated. The resulting factor structure resembled closely that which was anticipated.

An examination of Fleishman's data is of considerable interest. First, his intercorrelations are, in general, typically small and similar in magnitude to those published in related studies. However, correlations among clusters of tests which define factors are relatively high for this domain and sometimes greater than .50. These clusters can be easily identified by inspection. The reason why Fleishman has been able to achieve high correlations within clusters and low correlations between clusters is that tests measuring a single factor tend to differ from one another only in relatively minor details. For example, the two tests which define factor VI are visual reaction time and auditory reaction time; the two tests which best define factor I are both tapping tests but differ in the size of the circles used ($\frac{3}{8}$ inch and $\frac{1}{2}$ inch). The data give the impression that slight changes in test form produce substantial changes in factor loadings. This fact points to the very limited possibility of using such tests for prediction except where the response to be predicted occurs in a situation which closely approximates that of the test. This factor analysis reaffirms what previous studies have already established but presents a rather gloomy picture concerning the use of the variables as independent variables in prediction studies.

Other major factor analytic studies include one by Green *et al.* (33) in the area of reasoning. This study presents a clustering of reasoning tests similar to that achieved in a long string of previous studies. Just what each one of these studies achieved which its predecessor did not is far from clear. Marron (49) in reviewing such studies of reasoning abilities points out that they show some consistency in the variables they postulate, but their worth will be established only when measures highly saturated with each factor have been developed. Matin & Adkins (50) reported a second-order factor analysis of a 60-variable study of reasoning abilities. However, it is disturbing to find that the results do not seem to fit any theory related to thinking or problem-

solving that has yet been developed, and neither do they seem to provide a basis for a new theory. A more novel contribution in the development of intellectual variables is a study of categorizing or classifying behavior by Gardner (29). This class of variable has a resemblance to some of the intellectual variables discussed in connection with the Rorschach.

A newer field for exploration by means of factor analysis is the domain of originality. Wilson *et al.* (75) defined originality in terms of (a) uncommonness of responses as measured by weighting the responses of an individual according to the statistical infrequency of those responses in the group as a whole; (b) the production of remote, unusual, or unconventional associations in specifically prepared association tests; and (c) cleverness of responses, as measured by ratings of degrees of cleverness exhibited in titles suggested for short story plots. Seven measures of these three response characteristics were included with 46 other measures of creative thinking in a battery administered to adult subjects. Five of these seven measures of originality emerged with substantial loadings on one factor which, in accordance with custom, was tentatively named originality.

The measurement of individual differences in the general domain referred to as personality still remains woefully detached from the body of knowledge acquired about personality development. However, this field of individual differences retains an exuberant optimism. In this regard Cattell & Anderson (16) provide the year's most optimistic statement that in the area of projective devices "few recent advances have been so promising as that connected with music perception." This writer's own enthusiasm for the Music Preference Test was somewhat dampened on discovering that the Form A—Form B equivalence coefficients for the 11 scores (factors ?) derived from the test had a median value of .16 ($N=102$) in one sample and .26 ($N=71$) in another, and only 4 of the 22 correlations were above .40. This does not lead us to endorse either the optimism associated with this venture or the conclusion that "seven or eight factors alone be used as internally valid measures in routine applied psychology," for the median of the seven highest equivalence coefficients of those discussed is .38 in the one sample and .27 in the other.

The same spirit of optimism is found in an article by Cattell & Gruen (18) on primary personality factors in children and in an article by Cattell & Beloff (17) on the development of the I.P.A.T. Junior Personality Quiz which is presented as a test similar in basis to the 16 PF-test but suitable for 10- to 14-year-old children. Cattell & Beloff state that "it is only recently that the personality factor structure of 10- to 14-year-old children has become sufficiently clarified in terms of behavior ratings, questionnaire responses, and objective tests, to permit any truly informed test construction." This writer has doubts as to whether the personality factor structure of this youthful age group had become so well clarified. Cattell & Beloff admit that the "external or social validity" of their test needs to be investigated and

then as if grasping for some support for their position state "in terms of information theory, the fact that the test deals with so many demonstrated independent dimensions of behavior argues for the whole giving better prediction than any single, long, and reliable scale, because each factor brings new information." This argument is an illustration of the common logical fallacy of the excluded middle term.

Perhaps the uncertainty of the ground on which Cattell, Anderson, Gruen, and Beloff have developed these studies is most clearly brought out in French's (28) compilation of factor analytic studies in the area of personality which is similar to his previous compilation in the intellectual domain. In this monograph interpretation has been introduced only to a limited extent, and although it represents the most painstaking work and is exactly what it was intended to be, the present writer finds the mass of data overwhelmingly confusing. It is possible that a system for cataloging and indexing factors which Cattell (15) proposes might help greatly in organizing the data accumulated in this area. Perhaps also a part of the difficulty stems from the fact which French himself brings out in the statement, "Personality is concerned not with what the subject *can* do in response to an *easily* controlled stimulus, but what he *does* do in an uncontrolled complex situation." There is certainly serious question as to whether a genuine science of behavior can be built on a basis of studying responses to uncontrolled stimulus situations for almost any system of psychology seems to require stimulus variables as well as response variables.

An attempt to organize the findings of the numerous studies reported by French has been undertaken by Eysenck (25), a firm supporter of conventional methods of factor analysis. He tells in his introduction how the studies discussed in his book seemed to him to present a mass of contradictions, until he had an opportunity of discussing them with Thurstone. This conversation changed his pessimistic outlook and led him to attempt an organization of the data. Eysenck discusses personality in terms of traits and types. A trait is defined as a "co-variant set of behavioral acts," while a type is a group of correlated traits. At the type level, Eysenck believes that three dimensions have been established: Neuroticism, Extraversion-Introversion, and Psychoticism. At the trait level a number of concepts are offered such as persistence, primary suggestibility and level of aspiration, but some of these are offered on the basis of flimsy evidence.

Of particular import in recent applications of factor analysis are those in the domain of interests, since these variables have long shown promise for improving the accuracy of predicting responses to educational and occupational situations. A major enterprise in the factor analysis of interest variables was undertaken by Guilford (34) and his associates who performed a factor analysis on scores derived from 100 instruments of the inventory type. This study which is executed with painstaking and workmanlike skill and with patient and meticulous care illustrates so well the difficulties that the

psychologist must face in applying factor analytic techniques to measures of variables other than intellectual skill that it is instructive to review it from this aspect.

In this study, 100 inventory-type instruments were developed for measuring 100 postulated interest variables. It was hypothesized that these variables would cluster into 33 categories. Variables and clusters were selected mainly on the basis of previous factor analytic studies. Thirteen of the factors defined in the study were defined by clusters similar to those originally postulated, and 17 of the factors were considered to be the same in the two populations studied, one of which consisted of enlisted men in the Air Force while the other consisted of officers. Why these two groups might be expected to differ is not indicated.

The difficulties presented by this type of study do not lie in the mechanical features of the factor extraction process but in the interpretation and utilization of results. This is clearly illustrated by Factor V₁, described in general terms as a need for sympathetic environment. Variables which define this factor include those described as interest in cleanliness, need for reassurance from others, interest in meeting new people, interest in physical comfort, and possessiveness. These characteristics hardly seem to have the kind of belonging-togetherness which permits the attaching of a simple name to the factor which they "define." The attempt which Guilford has made to abstract some common property among these traits neglects the fact that interests may be related only because by a cultural coincidence they have been learned together. If it is assumed that interests are responses learned by reinforcement, the most useful interpretation of interest factors must surely be derived from the antecedent conditions that produced them rather than in apparent similarity of the objects, ideational or physical, towards which they are directed. Interpretation of factors must surely be based on a theory concerning how they are generated if such interpretations are to be profitable.

It is quite significant to point out in the present connection that Torr (70), who performed a factor analysis of 49 interest variables derived from seven different inventories, preferred to leave four of his rotated factors undefined rather than to force inappropriate definitions upon them, and two factors were described as response-set factors resulting from the form of the test items. The remaining six factors which are "tentatively defined" in terms of classes of activities could be as well, if not better, described in terms of sociological circumstances which, on the basis of the assumption that interests are learned, may be postulated to account for their existence. However, some such as Eysenck (25) who have carefully considered this problem believe that this difficulty is merely a reflection of the immaturity of the science of behavior and that the classification of end products of behavior is a preliminary step which must be taken before what are often described as causal laws can be discovered.

INDIVIDUAL DIFFERENCES AND ANTECEDENT CONDITIONS

The previous discussion of factor analytic studies has emphasized the need for understanding the process by which individual differences are generated. However, the difficulty of separating the effects of hopelessly confounded antecedent conditions, particularly with respect to differences in nature and nurture, resulted during the last decade in a declining interest in the area until the recent publication of *Intelligence and Cultural Differences* by Eells *et al.* (24) stirred up anew controversies which were familiar in the mid-thirties. Several papers have appeared which take issue with the conclusions presented in the latter publication. Eells (23) reiterated the conclusions of this study in a new article which shows no influence from recent criticism. Tyler (71) undertook a reanalysis of some of Eells' data and threw doubt on the latter's conclusions that while low status groups showed similar mean scores for both verbal and nonverbal tests, differences between these means increased with increasing status. A reanalysis of the data indicated that the phenomenon discussed by Eells *et al.* was a product of the fact that IQ units on the different tests are not equal. When the data were recomputed and such inequalities were corrected the phenomenon discussed by Eells disappeared. Similarly, nonlinearity of the regression of cultural status scores on IQ which Eells had discussed appeared to be a product of insufficient ceiling to the test.

Lorge (47) and Stenquist (65) attempted to evaluate the implications for selection of *Intelligence and Cultural Differences*. Stenquist's article seems to accept the argument of Eells *et al.* that low status groups have difficulty in understanding tests which reflect a middle-class and upper-class culture and will have similar and corresponding difficulties in profiting from a curriculum based on this same culture; hence, the school curriculum should be redesigned. However, serious doubt concerning the logic of this argument is raised in a study by Gordon (31) which demonstrated that antecedent conditions which affect test scores may not have a corresponding effect on advancement in educational programs and that it is necessary to be on one's guard when making assumptions that they do. These findings differ in some degree from those of Schultz (60) who compared the relationship between college grades and aptitude test scores for students from families of high, middle, and low socioeconomic status. The differences in the regression of grades on the Selective Service College Qualification Test for these three status groups were found to be insignificant, but this conclusion must be interpreted in the light of the fact that the status scores used in this study represented a highly restricted range.

What seems to the writer to be the most appropriate evaluation of the controversy pertaining to the Chicago studies comes from Lorge (47), who, accepting the data rather than the conclusion of these studies, states:

The authors have not made novel contributions to substantive knowledge, or to research design, or to procedures; nor have they contributed new interpretations of

established facts. They have, however, rekindled an interest in the question of the relation between status and intellect. Their data, indeed, can serve the purpose of giving a bench mark for the interrelation of these factors for the year 1946. In the amelioration of educational opportunity, in attempting to make "children more effective and happy in their lives," and in the widening of communications and interactions the relationship may be reduced. It is hoped that some researchers as enterprising as the Chicago group will bring in the evidence in 1971.

THE TECHNOLOGY OF DEVELOPING TESTS AND DERIVING SCORES

A substantial fraction of the literature usually reviewed within the purview of individual differences consists of contributions to the technology of test construction and of deriving scores from responses. The development of this technology is an essential supporting activity for research related to the discovery of laws of human behavior of the S-R type. The year has produced some interesting advances within this area of technology, as well as a general text on the subject by Bean (6), a general text on attitude and opinion measurement by Remmers (55), and a highly specialized text on the Rorschach by Phillips & Smith (54).

A notable landmark in the history of test technology in the publication by the American Psychological Association of a final draft of its report on *Technical Recommendations for Psychological Tests and Diagnostic Techniques* (2). This report has been prepared in co-operation with other interested professional bodies and represents a consensus of opinion concerning the information which test publishers should make available to test users.

Of particular importance to the test technologist is the work of Loevinger, Gleser & DuBois (45), who developed a technique for deriving from a heterogeneous group of items scoring keys such that each represents a relatively homogeneous subtest which yields scores minimally correlated with the scores derived from the other subtests. The method was previously described in government publications, but it is only through a recent article in *Psychometrika* that it has become accessible to most psychologists. In order to apply this method it is desirable, though not absolutely necessary, to obtain a complete matrix of the variances and covariances of the items in the heterogeneous pool. An alternative method is suggested by the originators of this technique which requires the computation of only certain submatrices. Whichever method is used the scientist must locate nuclei of items which have high covariances *inter se*, and then add to each nucleus those items which maximize the reliability of the resulting test as required by the Kuder-Richardson Formula 20. The procedure in developing the keys is mainly objective, but a certain amount of judgment must be introduced as when an item contributes to two keys and the investigator must decide whether to retain it in one or both of the keys or to reject it. One presumes that ultimately objective methods will be developed for making such decisions. The method has been applied to the problem of keying biographical information but could be appropriately applied to the keying of almost any heterogeneous

pool of items which permit the computation of item variances and covariances. The procedure, which has become known as the homogeneous keying technique, represents a major invention in the technology of test development, but like all techniques it does not provide any basis for the crucial matter of deciding which items to include in the original pool.

Loevinger, Gleser & DuBois are concerned with certain limited aspects of the relationship of item characteristics to the scores on the resulting test. A more detailed discussion of this problem is provided by Lord (46) in an article entitled "The Relations of Test Score to the Trait Underlying the Test," which, despite its title does not deal with the problem of the relationship of noumena to phenomena, or genotypic to phenotypic behavior. Lord is mainly concerned with predicting the parameters of the distribution of test scores from the characteristics of the test items and, like his predecessors in this field, often has to pursue his argument with little reference to the actual characteristics of test items. For example, he concludes that "U-shaped and rectangular distributions of scores can be obtained if sufficiently discriminating test items can be found." This is perfectly true, but outside of certain sensory discrimination processes, it hardly seems possible to construct items which have the necessary discriminating power. This kind of difficulty limits the applicability of the work of Lord as it has limited that of his predecessors. All too often these efforts become a science of events that never happen.

There is much to be said for inquiries leading to improvement in tests which have as their initial data actual item characteristics. This does not preclude theoretical developments but merely places them on a realistic foundation. Among the studies that have this as their goal is one by Ebel (21) who reported on investigation of a much neglected aspect of test behavior, namely the time taken to respond to each item. In individual intelligence tests, it has been customary with many types of test item to permit S only a limited time for response, but it has not been possible to control this time factor in tests administered to groups. Ebel adapted a technique developed by Blommers for measuring time per test item in a group situation and used this measure as an additional item characteristic for the building of more "valid" tests of arithmetic and contemporary affairs. The suggestion is that this is to be accomplished by assembling otherwise "good" items which have a short response time. In this way a greater number of test items can be included in the time available than would otherwise be possible. The suggestion is an interesting one, but it seems to disregard the fact which Ebel notes, but passes over, that the operation performed in solving a test item may change as a function of time. For example, many difficult space visualization items may be solved by verbal reasoning methods if sufficient time is provided. If the items for a test are selected on the basis of the speed of response, the resulting test may define a different variable from what it might if this selection factor were not operating.

Another approach to the improvement of tests is to experiment with item

form. Dressel & Schmid (20) disturbed by the fact that the usual multiple-choice item classifies respondents only into pass and fail categories, experimented with modifications of it which make it possible to derive a distribution of scores from the responses to a single item. These modifications did not seem to reduce the usefulness of multiple-choice items but evidence is yet to be presented to demonstrate that they are worth the trouble of incorporating in tests. Another attempt to do this by a more traditional approach is provided by Coombs (19) who revived the proposal of scoring each alternative to multiple-choice items. This approach does not appear to the writer to be too profitable because it appears to be most applicable to items which should have been written in a true-false format in the first place.

Although item analyses are commonly used for identifying ineffectual distractors, little is known concerning the value of this procedure for raising reliability. Zimmerman & Humphreys (78) studied the effect of omitting distractors which item analysis had shown to be nonfunctioning and found that this procedure resulted in a significant gain in reliability and a reduction of time per item. The study deals with the effect of omitting these alternatives and does not answer the question often asked by test technicians as to whether it is better to rewrite the ineffectual distractors, at the risk of writing even more ineffectual ones, or whether it is better to omit them.

The validity of test items under varying conditions has also been studied. Often S - R relationships where S is a test item are surprisingly unrelated to concomitant conditions which one might well hypothesize to affect responses. For example, Schultz (59) studied the item validities of a questionnaire designed to measure nonintellectual factors associated with college success under two different conditions. One was when the questionnaire was administered prior to admission as if it were an admission test and the other was when it was administered after admission. Item validity was measured in terms of the correlation between responses to the item and a measure of the discrepancy between college achievement as predicted from the College Board Scholastic Aptitude Test and actual achievement. Differences in item validities taken as a group were small and inconsequential under these two conditions, and there was a slight tendency for changes to be greatest in the case of the numerically largest validity coefficients.

SUMMARY AND OUTLOOK

In this chapter an attempt has been made to emphasize certain trends in the study of individual differences. It is believed that these trends represent a movement away from the study of mere concomitant variations of an R - R type and towards the study of rudimentary types of S - R relationships representing functional relationships or, if the reader prefers the term, causal laws. To some extent this represents an advance from a nineteenth century philosophy of science as represented by the British empiricists and the elder Pearson to a twentieth century viewpoint as represented by such varied thinkers as Russel, Pratt, Keynes, and Kantor. It is a change from an em-

phasis on studies of concomitant variation to an emphasis on the development of a body of knowledge which has structure. It also represents a change from a belief that variables exist to be discovered, much as oil is discovered, to an understanding that variables are convenient methods of description invented by the scientist. If this trend continues it may be expected that the study of individual differences will become progressively more an integrated part of systematic psychology with postulates and principles which refer more frequently to functional relationships between stimulus variables, response variables, and intervening variables and less to the formal properties of measuring instruments. However, it must be kept in mind that the study of such functional relationships has been made possible by the traditional type of psychometric research which has evolved measuring instruments characterized by properties which research instruments should possess.

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PERSONALITY^{1,2}

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In order to understand the continuously expanding field of personality study in contemporary psychology, one has to keep in mind that it stands, to a large extent, for a new approach to the "general" psychology of human behavior. Originally, personality study was opposed to psychology of "the generalized human mind," as general experimental psychology then was called (2, p. vii). It attempted "to depict and account for the manifest *individuality* of mind" (2, p. vii), i.e., its unique organization and personal development as differentiated from that of others. At present, however, personality study covers continuously increasing areas of "general" psychology of human behavior. The behavioral process, which for a long time has been studied as an abstract and "depersonalized" *S-R*² connection, tends to become incorporated again in the unity and continuity of the behaving personality. Human motivation, as discovered by medical or clinical psychologists, established the first bridge between the abstract behavior process and the behaving subject. Thus, this most important chapter in "general" behavior study has become a part of personality psychology. This is not only the case for the study of motivational structures and developments as a basis of the unique character organization in the individual, but as well for the processes and laws of human motivation in general.

More recently, the general study of cognitive processes tends to be included in personality psychology. The human behavioral response seems to be aroused, not by a "depersonalized" stimulus pattern, but by a meaningful life situation in the subject's personal "world," built up by perceptual or cognitive processes. In this way, the cognitive processes become incorporated in personality study, not only as influenced by motivational factors, but as an integrative function of personality as a whole. Thus, the "general psychology approach" to personality tends to become the study of human behavior in as much as behavior is considered a "personalized process," i.e., "personality behaving in its phenomenal world." In that sense, behavior tends to become identical with personality functioning. The "individual psychology approach" to personality continues to include research on personality measurement and assessment, on determinants of personality formation and development, attitudes and traits, character and personality structure, etc. The same trend in personality study manifests itself in European psychology, where it may be exemplified by a textbook by Lersch. The new edi-

¹ The survey of the literature pertaining to this review was completed in March, 1954.

² The following abbreviations have been used in this review: Ss (subjects); *S-R* (stimulus-response); TAT (Thematic Apperception Test).

tion of his manual in characterology (individual psychology approach) has mutated to a textbook in general psychology under the title *Aufbau der Person* (43).

This review will be concerned first with the "general psychology approach." Recent theoretical and experimental studies on cognitive factors in personality functioning and on specifically human motivation seem to be among the most promising trends for the further development of psychology. Among the studies concerning the "individual psychology approach," research on cultural and constitutional determinants of personality development will be discussed. Lack of space did not permit us to report on other important areas, viz. personality structure and personality assessment.

COGNITION AND PERSONALITY FUNCTIONING

Genetic theory of cognition and personality.—In the literature of the past year, the trend just mentioned manifests itself in a Symposium on "Cognitive Theory and Personality Functioning" by Scheerer (72), Postman (64), Leeper (42), Sweet (77), and Adams (1). Several ideas expressed there seem to be important for further research.

Scheerer (72) shows why learning theory needs more data and more accurate theoretical insight into the developmental stages of human cognition in order to explain personality functioning. As a matter of fact, cognitive theory conceives of behavior as relating to meaningful situations.

As distinguished from the S-R type of system, cognitive theory assumes that the organism is in commerce with its environment through the medium of selective representation. This means that the responses of the organism are not determined directly by proximal stimuli or by specific receptor-effector connections. Rather is it assumed that there is interposed a *central* process which represents features of the environment through phenomenal organization (72, p. 1).

The way in which the environment and the self are cognitively "represented" differs at the different stages of personality development. Thus, Scheerer argues, we have to determine the cognitive field of the child at each developmental stage in order to get better understanding of learning development and personality functioning. Consequently, learning theory can be applied to personality development only if a genetic theory of cognition is taken into account. These earlier ways of cognitive structuring of the life-space create certain adaptive behavior patterns which may persist in adult personality functioning (72, p. 9).

Concept formation and life-style.—Leeper (42), on the other hand, shows the importance of concept formation in personality functioning. He is impressed by the "tremendous degree" to which motivation and behavior in human life are determined by "representational processes" which produce for each person the apparent realities of his behavioral world. In fact, however, these representational processes are representational habits. They reflect properties in the behavioral world which are not present in the afferent neural

material, i.e., in the stimulus situation, but are represented merely on the basis of past experiences. The point Leeper makes with regard to these representational habits is that they are not to be conceived as merely concrete traces of the concrete experiences of the learner. In considerable degree they are concepts. This means that the learner "typically out-runs or out-distances" the data or experiences on which the representational habit is based. As a concrete example, Leeper shows how a boy, having experienced his mother saying, "put back that magazine, you can't read it anyhow," may tend to form representational habits about himself and his environment which transcend the factual data on which they are based.

Leeper also emphasizes the fact that cognitive theory does not imply all of these representational processes to be conscious. Moreover, representational habits may resist further cognitive information. In conclusion, Leeper believes that cognitive theory tends to support the basic type of personality concept as involved in Adler's theory of "life style," and more recently in Rogers' theory of "self-concept" (69).

Postman (64), in the same Symposium, stresses the perceptual approach to personality. This aspect of cognitive theory will be dealt with in a special section of this chapter.

Ego psychology.—The same cognitive trend in connection with personality motivation is currently represented in psychoanalytic ego psychology. Along with the dynamic aspects of the ego, as manifested in the mechanisms of defense, the "most encompassing quality of the ego," Wyatt (84) claims, is its "continuous, effortful, goal directed, self-experienced activity,—what we mean when we speak in common parlance of 'thinking'" (p. 147). This way of conceiving the cognitive function of the ego is different from Freud's conception. Dynamic and cognitive elements in the behavior control of the ego are integrated.

Thus, the quality fundamental to the ego is not only that it controls motility or that it sets up defenses against instincts (Freud, S. 1927); all these are but facets of the *cognitive function of the ego: differentiation and organization*. Whatever dynamic constellation is postulated to account for behavior (or for the stalling of it) is a function of the fact that the ego differentiates, that the ego reasons (84, p. 147).

French, in his important recent books on *The Integration of Behavior* (26, 27) bridges the gap between the conscious and the unconscious levels of cognitive processes in their relation to motivation. The work of French (27) and Boss (10) on dream interpretation is most significant for the rehabilitation of the cognitive components of motivation in psychoanalytic thought.

In a contribution on *Psychoanalysis and the Unity of Science* (28), Frenkel-Brunswik also emphasizes the development of psychoanalysis in the direction of including cognitive factors. But she believes that

psychoanalytic expansion in this direction has been more programmatic than real, and (that) there are a number of problems which can be solved only by an explicit integration with psychology and sociology. The conceptual tools of psychoanalysis

just are not sufficient fully to explain rational and social behavior. . . . Some of the resistances against admitting psychoanalysis to the circle of respectable sciences may well be based on an unrecognized transfer from the realization of the failure of psychoanalysis to deal adequately with reasoning behavior (28 p. 298).

In the context of Jungian analytic psychology, the importance of cognitive experience in ego development is stressed by Lewis (45). Analyzing group play in children between 8 and 12 years old, she shows how the affective-cognitive experiences of the outer and inner reality help enrich and integrate the ego.

European trends.—With regard to the cognitive aspects of personality functioning, some important trends have developed in European thought which, up to now, do not seem to have influenced American psychology. Their origin and actual frame of reference is twofold: philosophical phenomenology and existentialism, and a new kind of "anthropology" developing chiefly in medical and psychiatric circles. The latter is inspired by the former philosophic movement, and its problems originate in the thesis that a more profound insight into the nature of human personality must be gained in order to understand and to be able to cure personality disorders. Therefore it is frequently called "Medical Anthropology" (16, 30). The profound difference between "existing as a sick man in the world," as opposed to "existing as a well man," has been analyzed, for instance, by von Weizsäcker (81). But the "anthropological meaning" of illness, i.e., the human significance of illness for personality as a whole, can only be understood, it is claimed, if the most essential relations with the "world" (including those with fellow men and God) which constitute human personality are clarified (8, 25, 30, 35, 63). Thus, a great deal of European personality study develops in this philosophically inspired medical context. Lack of self-understanding and of the understanding of the meaning of life is considered frequently to be the origin of neurotic disorders. Therefore, "anthropological psychotherapy" is in need of a comprehensive personality theory in order to be able to liberate the patient by means of a sounder self-concept (30). While the therapeutic theory of "self-concept" in America has developed on a purely psychological and biological basis, in Europe it has frequently been elaborated in a philosophical context. The same trend exists in some contemporary schools of psychoanalysis in Austria (15, 24).

As to the phenomenological trend in European personality theory, only a very few ideas will be mentioned here. They are directly related to the cognitive theory of personality functioning. In the previously mentioned symposium (1, 42, 64, 72, 77), and in the work of cognitive theorists in general, one is surprised to see the perceptual world is considered as a cognitive "representation" of the outside reality existing in the organism. It is important to realize that man's perceptual and behavioral world, as it psychologically exists for him, is by no means a "representation" in him of what exists outside him. From the behavioral standpoint, perception is a direct contact

with the world itself; it effectuates "man living in the world," and not "the world represented or duplicated in an enclosed human organism." This erroneous conception is the consequence of introducing in a behavioral science the standpoint of epistemological and physiological research about the cognitive process. In fact, from the physiological point of view, cognitive processes are "organismic states which the organism carries about in the nervous system" (77). And philosophical epistemology is seeking for "reality" behind the phenomenal and behavioral percepts. From the psychological point of view, however, there is no doubt that the perceptual world is not to be conceived of as a "representation." Cognitive behavior and personality theories will always act on a "distant scene" as long as the contact with "things themselves" is not re-established.

An important consequence of this deep-rooted "representational theory" of perception concerns the concept of personality itself. Personality as a rule is conceived of as a "closed," inner organization of attitudes, traits, aptitudes, and behavior consistencies. Personality, no doubt, constitutes such an inner organization of traits. But the most important feature of the specific modality of "existing" which characterizes "personality" seems to consist in the fact of its having an outlook on the "world," its not being blindly enclosed in a functioning organism. Thus, the original and fundamental personality structure is to be conceived of as an "ego-world unity," and not as a merely internal organization. In this context Scheerer's previously mentioned considerations on developmental stages in the cognitive field gain more importance. Personality development precisely means new organization of the cognitive and affective "ego-world" relationship.

Rigidity.—Among the many concepts of special importance for a cognitive theory of personality and experimentally studied in recent literature, the notion of rigidity has been chosen for review. Research on the "depersonalized" behavior process has revealed that a learned response may be repeated in a new situation in spite of the fact that information given to the subject reveals that the new situation "demands" another response. This kind of functional fixedness, as produced by reinforcement, has been considered, in learning theory, an important argument against the cognitive theory of learning (76). In recent years, the incapacity or reluctance of the subject to adapt himself to the demands of a new situation has been studied in the context of personality functioning and in a more general experimental setting. It is now commonly called rigidity and is studied in its cognitive and dynamic aspects. Some psychologists conceive of it as a personality dimension. In any case, more precise knowledge about rigidity and flexibility in behavior adjustment, problem solving, and dynamics, is of vital importance for a cognitive personality and behavior theory. As Leeper pointed out, fixedness and rigidity are not opposed to cognitive theory, since cognitive representations themselves may manifest them (42).

The thesis that rigidity, as shown by the *Einstellung* method of Luchins, is a "general response characteristic that pervades all aspects of an individu-

al's behavior" (17) was not proved (62). Goodstein, on the other hand, concludes that intellectual rigidity is not to be considered a unitary trait (31). Factor analysis, it is claimed, confirms the view that rigidity is a generalized feature of psychotic personalities and that convulsion shock therapy produces a decrease in rigidity (66a). Goodstein (31) fails to find any significant relationships between intellectual rigidity as measured by three tests (an adaptation of Luchin's water-jar problems, the Shipley-Hartford retreat-scale, and the anagram problems) and extreme social attitudes. Some evidence, however, has been found in favor of the relationship between rigidity and ethnic prejudice and the Frenkel-Brunswick concept of "intolerance of ambiguity" (20, 53).

Several aspects of the problem of rigidity are re-examined by Levitt & Zelen (44). If rigidity is defined as "the inability to change one's set when the objective conditions demand it" (68), the *Einstellung* test of Luchins (47) does not seem to measure real "rigidity." In fact, the solution of the second series of water-jar problems does not "demand" the shift to the presumably simpler method, since it is ascertained that this so-called "short" solution of the problems does not lead in fact to faster work, even under incentive conditions (44). As to the problem of the relationship between ethnocentrism, prejudice, and rigidity, it is found that the ethnocentrism as measured by the California E-scale does correlate with rigidity in "free" conditions, but the relationship disappears when a reward for speed of work is offered (44). Moreover, an interesting attempt is made to explain some experimental data on rigidity in terms of more general concepts: conformity and overgeneralization (44). The same trend will be examined later in this chapter with relation to perceptual defense. The authors argue that

It is known that one of the aspects of ethnocentrism is *conformity*—to what the milieu seems to demand. We may think of the consistent use of the set solution as an example of conformity of this kind.

On the other hand,

Studies of the prejudice suggest an alternative explanation which we term the overgeneralization hypothesis. . . . Kutner (39) and O'Connor (59) have shown that overgeneralization and poor inferential ability are characteristic of the prejudiced. . . . We may think of the ethnocentric as leaping to the conclusion that the set solution is the only solution, in much the same fashion that he concludes that all Negroes are alike. . . . Perhaps overgeneralization is the cognitive manifestation of the more dynamic desire to conform (44, p. 578).

In conclusion, it will be necessary to study in more detail the cognitive and dynamic processes underlying "rigidity" in order to get insight into its role in behavior and personality adjustment. Moreover, the concept itself needs clarification.

HUMAN MOTIVATION

One of the basic problems still facing motivation theory is "bridging the

gap between the bodily needs of the infant and the adult's desires to succeed or fail, to help or hurt, to be loved or hated" (13, p. 99). Some articles have been published in this area by psychologists who are looking for a solution of the problem in the learning process, i.e., the acquisition of motives and affective states (13, 74). Others, on the contrary, are seeking for a new framework in order to emancipate motivation theory from a few ingrained concepts which, in their opinion, seem to prevent a sound development of psychology in this field.

Need reduction.—One of these deep-rooted concepts frequently called into question in recent publications is that of human motivation as a matter of "tension reduction" and "equilibrium seeking" (3, 33a, 49, 50, 51, 57). On the basis of their experimental work on the achievement motive McClelland *et al.* (49) found the concept inadequate. Allport finds it "scarcely consistent with all the known facts" (3, p. 108, 117). Attention also was called to the fact that the chief characteristic in the dynamic development of man is his inability to resign to equilibrium and rest. The main dynamic fact to be explained is the constructive impulse, planning new realizations and thus destroying the state of equilibrium as soon as it has been reached (57, p. 247). As a consequence, the great bulk of motivation in a healthy personality is constituted by a "forward thrust into the future"; it is by no means adequately represented by an exclusive study of the unconscious levels of motivation and the history of the past life (3). The very fact that human needs are elaborated at a cognitive level causes them to exist and to be active as "projects" and "open task systems," i.e., "cognitive structures in a status of tension" which chiefly regard the future (58, p. 426). Therefore, Allport claims, human motivation in well-integrated people should be studied not only by means of projective or indirect techniques, but also on the level of conscious strivings by means of "direct" methods (3). Psychoanalytic ego psychology, giving more attention to cognitive functions and ego dynamics, as was mentioned previously, also stresses the importance of studying both the conscious and the unconscious levels of motivations (38, 84).

Achievement motive.—An important experimental contribution to the study of specifically human motivation was published last year by McClelland and his collaborators (49). The human need for achievement and the need for affiliation have been the chief topics of research in this group. Work on the need for affiliation was published in 1952 (75), but the main results of a five-year research program have been published now in a volume, *The Achievement Motive* (49). The work of this group constitutes an interesting illustration of what has just been mentioned about deep-rooted concepts in motivation theory. The authors say,

Our basic approach was simple: it involved attempts to alter the content of fantasy experimentally. . . . We began with the effects of hunger on imagination. . . . But we wanted to go beyond such simple variables in the conviction that, if we are to understand human nature, we must start operating in the laboratory with the kind of motives which actually are important in the lives of human adults (49, p. 319-20).

These adult motives, however, were approached, in the first stage of the research, with hypotheses adopted from the study of biological needs. Achievement motivation, it was supposed, could be aroused and graduated in the same way as hunger drive, i.e., in function of the amount of deprivation (failure). Only gradually it was realized that instructional cues, i.e., cognitive factors in the situation, were essential here.

As to the method used, data for measuring the achievement motive were collected by a modified form of the thematic apperception method called "thought sampling." The Ss² are invited to imagine stories about four pictures shown on a screen.³ The stories are registered in a standardized form of written answers to a set of four questions: (a) What is happening? Who are the persons? (b) What has led up to this situation? That is, what has happened in the past? (c) What is being thought? What is wanted? By whom? (d) What will happen? What will be done?

Before the subjects are presented with the four pictures, the achievement motive is aroused in various experimental conditions. A 12 min. anagrams test is administered with instructions which tend to minimize, to force up (by reference to leadership and intelligence), or to keep normal the ego-involvement and the need for achievement of the subjects. Moreover, success or failure feelings are experimentally induced. The achievement motive is supposed to be aroused at different degrees in these various conditions. Immediately thereafter, but without any other connection with the preceding test, the projective technique just mentioned is administered by another experimenter. The purpose is to examine the effects of the differential condition in achievement motivation during the previous test on the imaginary stories.

What is considered crucial in scoring the stories for achievement motivation is "detecting some sign of involvement such as directly stated affect in connection with evaluation of performances." (For instance: "he wants to become a successful businessman," he is unhappy because he flunked the exam," etc.⁴) It is shown that the amount of achievement imagery, as here defined, is significantly influenced, in male college men, by the arousal of the achievement motive during immediately antecedent testing. An index of the subject's need for achievement is obtained by adding the number of different types of achievement imagery appearing in his stories.

The authors are aware of the numerous questions aroused by their procedure (pp. 325-26). The most important is whether the achievement imagery scores obtained are really to be considered a measure of motivation. Several

³ One of the series of pictures comprises the "Father-son" card and the "Boy with vague operation scene in background" from Murray's TAT³ along with "Two men ('inventors') in a shop working at a machine" and "Boy at a desk, an open book in front of him."

⁴ McArthur's article (48) concerning the problem of self-projection in the TAT stories and the principles underlying McClelland's scoring system chiefly relates to the 1949 contribution of McClelland.

behavioral indications tend to prove they are. In fact, people with a high achievement imagery score are found to be more active in accomplishing test-tasks; they set a higher level of aspiration, etc. On the other hand, the achievement index fails to correlate with two important validating criteria.

No matter how he is asked to do it, a person does not estimate his achievement motivation in a way which will correlate significantly with his achievement score index. Furthermore, a clinical judgment of his achievement motivation by outsiders—that is, in this case, by a psychiatrist and a clinical psychologist working together—also does not agree with the index score (p. 327).

The authors

are inclined to interpret these negative results as meaning that people's perception of achievement motivation and achievement motivation itself are two different things (p. 327).

The present writer thinks, however, that some fundamental questions are to be discussed with relation to the method used.

The basic methodological principle involved in this research is the Freudian hypothesis "that an excellent place to look for and measure the effects of motivation is in fantasy" (49, p. 3). This is certainly true in cases where motivation or need have not found a sufficient outlet on the level of reality. It seems not sufficiently proved, however, that normal people who succeed in realizing their need for achievement in every day activity will be urged to abreact their motivation in artificial imagery. Fantasy and associative thought processes seem to have a quite different function in maladjusted people as compared with the well-integrated personalities. For the latter, imaginative processes are related to the ego structure or to the dynamic core of personality inasmuch as they are functionally incorporated in "realization projects." Purely artificial imagery, in an experimental setting, may remain rather alien to the real motivation structure of a well-adjusted personality. Therefore, it is not self-evident that a normal man "reveals the content of his dominant thoughts" in experimental imagery as it is assumed here (49, p. 194). In any case, if imagery is to be used it may be desirable to integrate it in a real achievement process.

As to the motivation theory, the authors conceive of the achievement motive as developing out of growing expectations. The child for instance, "develops gradually certain expectations about what self-locomotion or a toy car will do for him and gets pleasure from confirming these expectations as long as they remain somewhat uncertain" (49, p. 78). In order not to get bored, the child "must continually work with more and more complex objects or situations permitting mastery" (49, p. 64). In adult people, expectations built up over a whole life history may be: "doing a good job, being a professional man, etc." (p. 66). Situations involving discrepancies between expectation and perception are sources of primary, unlearned affect. Affect is considered the basis for motives.

The merit of this expectation theory of motivation as developed by

McClelland consists in its stressing the social and cognitive factors involved in the concrete forms of human motivations. It seems difficult, however, to explain why "expectations" such as "doing a good job" or "being a professional man" are able to arouse such an astonishing amount of motivation and effort, while some other lifelong expectations on the purely cognitive level are abandoned easily in face of contrasting reality. Provided that no deep personality commitment is involved, a scientist, for example, upon accidentally discovering some new facts, may easily abandon not only life-long but centuries old concepts and expectations not in accordance with these new facts.

Fundamental dynamic orientations seem to be implicated in some "expectations." McClelland observes that expectations must remain "somewhat uncertain" and that situations "permitting mastery" are required. This points to the same difficulty as experienced by Allport (2, p. 204). Only "mechanism-on-the-make" or "in the process of perfecting" have motivation power. Once "mastery" is gained and expectations are "certain," no affect and no motive are aroused. That is precisely the important fact to be accounted for. Some fundamental and dynamic personality orientation seems to be implicated here and in any other form of motive or interest.

Fundamental needs.—The fundamental personality orientations just mentioned are not to be conceived of as concrete forms of instincts which, progressively, are canalized or developed into adult motives. As Nuttin has pointed out in his *Psychoanalysis and Personality: A Dynamic Theory of Normal Personality* (57), they should rather be thought of as general and vague directions discovered in the various ways man tries to go along with his fellow men and the behavioral world. Personality, as mentioned previously, is to be considered as an "ego-world unity." Personality functioning, i.e., behavior, consists in building up a concrete pattern of this "ego-world" constellation. Fundamental needs, it is pointed out, are to be conceived as behavioral orientations in these "ego-world" interactions which develop on the biological, the social, and the "existential" level. They constitute the most general modalities of relationships which the individual always seems to need in the innumerable concrete forms of behavioral contact with the various types of "environment." The assumption that the individual "needs" these modes of relationship is deduced from the fact that disturbances and anxieties arise as soon as they cannot be realized.

The achievement motive (49) and the need for affiliation (75), if broadly conceived, seem to illustrate two of these frequently recognized personality orientations or fundamental needs. They are the experimentally approached aspects of the "two-directional" orientation mentioned by Bronfenbrenner in his 1953 review (11) and described in various ways by different authors (5, 57, 67). They are labelled self-determination, self-realization, actualization, and need for consistency on the one hand; and self-surrender, contact, and universal integration on the other. It has been pointed out that both dynamic orientations can be identified on the biological, the social, and the

"spiritual" or "existential" level of the human relations with the environment (57, pp. 205-42). They are also intimately related to each other, and both seem to be present in almost any concrete motivation structure. Self-realization, in fact, is possible only in and by self-surrender. This contact with others, however, is done by some people and in some circumstances in a more "constructive" or in a more "defensive" way (love versus aggression). These contrasting ways may be considered as partially learned and partially determined by constitutional factors.

As these fundamental personality orientations are very general trends to be discovered in, and abstracted from, the great variety of concrete behavior patterns, it is obvious that they may be conceived of, and formulated, in very different ways. They also may be considered at different levels of "generality" or abstraction. Thus, for instance, the exploratory drive may be eventually conceived of as a manifestation in some people (more in males, for instance, than in females) of the fundamental need for self-actualization. Self-actualization, as just mentioned, means at the same time a more extended and more profound insertion of the individual in his biological, social, and universal "world" by means of physiological, cognitive, and all other kinds of interaction. As a matter of fact, the individual is not always aware of this general orientation underlying his concrete and immediate motives. This unawareness of the fundamental need underlying a concrete motive is frequently meant when the term "unconsciousness" is used in connection with motivation.

The necessity to emphasize a behavioral concept of needs, based on the previously mentioned "ego-world" or "organism-environment" unity of living beings, can be illustrated by two recent articles of Harlow (32, 33). Harlow makes a plea in favor of external stimulation as the primary motivating agency, as against the internal and homeostatic drives which are in fashion today (52). Curiosity-investigatory or exploratory motivation and manipulative propensities, he argues, are not second-order or derived drives conditioned by hunger, sex, or any internal drive. From the behavioral point of view, however, the so-called internal drives, such as hunger or thirst, are also needs to deal with the environment. The hunger drive of a rat in the maze, or of a man in the street, is a propensity to behavioral relationships with the environment. Stomach contractions or blood composition may be the physiological basis of a drive, but they are not the drive in a behavioral sense (58, p. 434). Higher degrees of the same physiological conditions, in starvation for instance, frequently cease to result in behavioral drives.

Thus, "internal" as well as "external" drives are always to be considered, in behavioral sciences, in function of the behavioral world. However, among the "organism-environment" relations which constitute human life, some do not develop on the level of biochemical interaction, as hunger does; they rather exist, for instance, on the level of perceptual or cognitive activities (50, 51, 57). Needs relating to interactions of this kind, as is the case with exploratory drive, do not refer to such manifest physiological stimulations

in the organism as hunger does, although internal as well as external conditions and stimulations are implicated in their action. There is, of course, no reason at all to postulate that needs developing at the level of cognitive and social interaction "derive" from needs at the level of biochemical "organism-environment" relationships. This makes no more sense than saying that cognitive activities "derive" from metabolic processes. As a matter of fact, the level of cognitive "ego-world" relationships, and the needs relating to them, develop in later developmental stages of the organism than metabolic processes do. This, however, does not implicate any "derivation" in the strict sense.

On the other hand, a "non-directional" theory of motivation and drives has been expounded by Brown (13) in the context of an *S-R* conception of behavior in the University of Nebraska Symposium entitled *Current Theory and Research in Motivation* (12). Direction of behavior here means that in a given situation, S_1 , the subject responds with R_1 . This direction is determined by the innate or acquired *S-R* connections. Thus, guiding or directing functions in behavior are reserved to "habits." Drives, on the contrary, have the functional properties: "(1) to activate or energize latent reaction (associative) tendencies; (2) to reinforce responses whose elicitation is followed by a reduction in drive; (3) to function as a punishment whenever abrupt increases in drive occur following a response" (13, p. 18). It is denied that drives *qua* drives are directed or are able to direct behavior. It is denied too that individuals can acquire drives for any specific goal object. What individuals do acquire are habits. As to the "supposed acquired drives" for particular objects, Brown proposes, "as a tentative solution," that they may be learned tendencies to be anxious (discontented, insecure) in the absence of those objects.

On this hypothesis, stimulus cues signifying a lack of affection, a lack of prestige, insufficient money, and the like, are said to acquire, through learning, a potentiality for arousing a state of uneasiness or anxiety having the functional properties of a drive. Thus this learned uneasiness would function to energize whatever behavior is directed toward the securing of affection, prestige, or money; and its reduction, subsequent to the achieving of those goals, would be reinforcing (13, p. 19).

It is not clear, however, why anxiety or insecurity is primarily aroused when no fundamental need is implicated.

MOTIVATION AND PERCEPTION

The great affluence of studies in the field of "perception and personality" seems to have facilitated a lack of criticism in drawing conclusions on motivational factors in perception. During the last year several efforts have been made to develop a more critical attitude in this field (7, 9, 41, 65, 66).

Criticisms.—Several experiments dealing with relationships between hunger and perception were criticized by Lazarus *et al.* (41) on the basis of the fact that they do not offer an adequate criterion of perception in their

measurements. In fact, perception in these experiments is considered "with virtually no reference to an unambiguous stimulus or with any consideration of perceptual accuracy." (41, p. 327). The way in which affect is measured in experiments on perceptual defense is questioned by Aronfreed *et al.* (6).

The problem of the influence of value on apparent size was re-examined by Bruner himself in a new experiment with much more methodological precaution (14) and also by Beams (7). Efforts were also made to analyze certain determinants of size over-estimation in children in terms of reinforcement and extinction (39a).

The main attack, however, came from Postman who made a general attempt to explain the new findings on motivated perception in terms of the general laws of behavior and learning (65, 66). He first vigorously objects to the "phenomenological" definitions of stimuli (describing, e.g., the perceptual stimulus as a "smiling face"), and he is right in pointing out that these phenomenological descriptions are responses of the organism (66, p. 63). The concept of perceptual response disposition is introduced to point to the consistent ways in which a given personality seems to categorize objects and events. These categorizing and organizing dispositions are reflected in the different though consistent response classes of a personality and are inferred from them. In a cognitive theory of personality, Postman claims, these perceptual (and mnemonic) response dispositions are to be considered the fundamental units of analysis (64). A person's system of categorizing, in Postman's sense, corresponds to what Tolman has called the "belief-value matrix" (79). Experimental evidence was given by Gardner (29) suggesting that individuals are characterized by unique categorizing response dispositions.

As to the motivational factors in perception, Postman intends to reinterpret the data in a way analogous to what has been done in the field of the law of effect. All agree about the fact that administering a reward may be an important condition in learning, but according to some reinterpretations of the law, it is not reward *qua* reward which is the causal factor. Reward is believed to create conditions for the operation of more fundamental determinants of learning (66). In the same way, perceptual defense, i.e., the elevated recognition thresholds for unaccepted or threatening stimuli (e.g., taboo words), may be a result, to a large extent, of the effects of frequency in the use of some words on perceptual recognition. In an experiment to test this hypothesis (65), a series of taboo words and neutral words was presented tachistoscopically for recognition. By means of the Thorndike-Lorge word count, the familiarity of both types of words was equated. Moreover, four different sets of instructions were used in order to influence the attitudes of the subjects with regard to the taboo words (facilitation versus inhibition). The experimental results failed to provide any support for a mechanism of perceptual defense. The thresholds for taboo words were even somewhat lower under all conditions. Preliminary instructions, too, have considerable influence on the relative thresholds. This suggests, as is confirmed by Bitterman & Kniffin (9), that differences in threshold of recognition can also be

understood, partially, in terms of "differential readiness to report." Moreover, there is some evidence from research on the law of effect showing that rewards and punishments influence the acquisition of perceptual response dispositions. Their influence is interpreted by Postman in terms of emphasisers and not of need reduction (66). Thus, Postman claims, the analysis of motivational factors in perception is possible without appealing to *ad hoc* mechanisms. Delucia & Stagner (18) are also in favor of this thesis.

Perceptual behavior may be usefully conceptualized in terms of learned response dispositions whose acquisition and performance are governed by general principles of associative learning such as frequency, recency, and effect. . . . There is little evidence for direct sensitizing effects of motivational conditions on perception. . . . These conclusions take us a long way from the simple and facile conception of the perceptual process as subservient to the wishes and needs of the organism (66, p. 99).

It is added, however, that

analysis in terms of general principles need not in any way detract from the existing empirical evidence for motivational selectivity in perception (66, p. 100).

Postman's effort to integrate the experimental data on motivated perception with the classical principles of learning is a very important one. The law of parsimony, however, should not prevent us, as Harlow warns, from taking into account the full complexity of all facts in this field (12, p. 109). Many of the most recent experiments in this area will not easily be explained in this way. To give only one example, a recent experiment of Rosenberg (70) proves that compulsive neurotics show a greater tendency to err in the direction of symmetry than normal people do when confronted by ambiguous figures which fall short of symmetry. These findings join some of Fenichel's hypotheses (23) concerning perception in compulsives based on clinical analysis. Postman's shift to an explanation in terms of frequency, recency, etc., also makes less clear the bearing of his own previous statements concerning the perceptual approach to personality (64). Personality factors, however, may be involved in the differential frequency with which some words are used by different people and still more in the influence of the law of effect in perception.

Two new approaches to the personality-perception problem should also be briefly reported. The first is a major contribution to the experimental study of personality "through perception."

Perceptual space orientation and personality.—An extensive report of a 10 year research program on perceptual space orientation and the personality characteristics involved was published by Witkin and his co-workers (83). The main body of the research may be summarized as follows.

The subjects were presented with a perceptual task of space orientation. The three tests of space orientation used were the rod-and-frame test, the tilting-room-tilting-chair test, and the rotating room test. In each of these tests the subject has to detect the upright. He is required to indicate this location by adjusting an object to a position in which he perceives it as verti-

cal. The object to be located is his body, a rod, or the field. In each of the three orientation tests, the subject may locate the upright according to the axes of the visual field, i.e., mainly with reference to the outer field (field-dependent perception), or mainly according to sensations from his own body (independent perception). Thus, the subject, it is assumed by the authors, is "confronted with essentially the same type of situation that people frequently encounter in every day life, where it is necessary to choose between standards based on one's own impulses and feelings and standards derived from environmental pressure" (p. 41). Here, the hypothetical relationship between the perceptual orientation task and personality factors is already clear. On the other hand, adequate orientation toward the upright is of continuous importance to a person in his behavioral contact with the environment. The authors mention that difficulty in establishing their position in the orientation tests led some subjects to feel extreme distress. This also shows the deep involvement of personality in this perceptual performance. Finally one's own body is directly employed in orientation and, as is well known, the body bears a close relation to the self (p. 14). On the other hand, the methods used in measuring personality organization involved the clinical interview, the TAT, the Rorschach test, and the Figure-Drawing test.

As to the results, only a very few headings can be mentioned here. They were revealed in an intensive study of a group of young, normal adults, and confirmed in studies of children and psychiatric patients. The nature of the individual's relation to his environment and other people was found to be particularly relevant to performance in the perceptual orientation tasks. Passivity, in the sense of inability to function independently of environmental support, absence of initiating activity, and readiness to submit to authority, is associated with "field-dependent perceptual performance." On the other hand, activity, as defined by the opposite characteristics and the power to struggle for mastery, is associated with "independent or analytic perceptual performance." A second group of relevant personality characteristics represents the nature of the individual's handling of his own impulses and strivings. Field-dependent perception is observed most often in people characterized by "lack of awareness of inner life, fear of aggressive and sexual impulses, and poor control over these impulses. Along with these characteristics . . . we find evidence of considerable anxiety, combined with difficulty in regulating it" (p. 468). The opposite characteristics are displayed prominently by independent perceptual performers. A third group of personality features represents the kind of self-concept and self-evaluation the subjects have. The trend associated with field-dependent behavior involves "low self-esteem, difficulty in accepting oneself, and low evaluation of one's body." The other trend, related to analytical perceptual behavior, involves the opposite features (p. 469).

No doubt, the methods used and the results obtained in this 10 year research by Witkin and his co-workers are of great importance for the joint fields of experimental and clinical personality study. The present writer only

ventures one remark. The perceptual orientation tests used in this research place the subject in a difficult problem and conflict situation where the task to be performed goes far beyond what is usually involved in every day perceptual responses. Therefore, the way the subject solves the kind of "ego-world" conflict with which he is presented in these situations is most significant for the "ego-world attitude" of the perceiver. It is not evident, however, that the personality characteristics related here to field-dependent and field-independent perception apply to object-field relationships in ordinary perception and to perceptual responses in general as involved in every day behavior. In other words, this stimulating research seems to be more important as an experimental perceptual approach to personality than as a study of personality factors involved in perceptual behavior in general. Thus, the author's title of the book is very accurate: *Personality Through Perception*.

Perception of success and failure.—Another series of experiments on perception relating to the self-concept was published by Nuttin as the first part of a book concerned with *Task, Success and Failure* in their relation to personality and to learning (58). Different groups of adult subjects and children were given, individually, a series of short tasks. Successes and failures were induced experimentally and communicated as such immediately after each task, so that the subject was clearly aware of his successive outcomes. The problem examined was the possible influence of certain personality factors on the perceptual organization of such a series of successively experienced successes and failures; whether, under certain conditions, success, for example, might tend to become the perceptually dominant element in the series as a whole, although objectively successes did not dominate.

Among other things, it was found that large individual differences in the perception of dominance of successes or failures are associated with the overall "optimistic" or "pessimistic" attitude of the subject toward himself or his activity in general. These general personality attitudes of the subjects were assessed on the basis of concordant ratings by three long-term observers for the normal people, and on the basis of a psychiatric diagnosis for depressive and manic patients. Ego-involvement too was found to be correlated with systematic changes in perception of success.

On the other hand, accumulation of failures or successes before or at the beginning of the experiment results in a systematically distorted perception of the series as a whole. Distortion is not according to the "law of contrast" but according to what could be called a "law of constancy." In fact, accumulation of successes at the beginning of the experiment results in an overall impression of "more successes" although, objectively, failures dominate in the subsequent series. Previous accumulation of failures, on the contrary, results in the opposite perception.

As to the theoretical interpretation of these results, only a few remarks relating to the previously mentioned "expectation theory" of McClelland (49) will be recorded here. The author examines the possibility of an explanation by the concept of "expectation." Different kinds of expectation are dis-

tinguished according to their more or less dynamic implications (58, pp. 148-50). It seems probable, in fact, that the over-all "optimistic" individual, as well as the subject who yields an accumulation of successes in the beginning of the experiment, is expecting a large number of successes. But why, the author asks, does the relatively larger number of failures which follows not produce rather a strong contrasting effect against the background of expected successes and the high level of aspiration developed in such circumstances? Instead of producing a contrast effect, the subject maintains his previously established "optimistic" or "pessimistic" impression, and continues to "perceive" the high number of successes or failures he "expects." Thus, the author argues, a state of expectation which systematically tends to distort reality in order to confirm what is expected must be supported by a dynamic orientation. The need involved in some of the results seems to be a need for ego consistency (or consistency of the self-concept). A theory is developed in which perception of personal achievements is conceived of as a regulatory mechanism in service of this need in poorly integrated personalities. The differential attitude of well-integrated and poorly-integrated persons toward success and failure is emphasized.

Perception versus other cognitive functions.—The term perception is being accepted in American psychology in an ever broadening sense. There are even examples of abuse, in the present writer's opinion, in the recent literature. Sweet (77), for instance, emphasizing the important "dimension" of distance in cognitive processes, uses the terms "indirect perception," "abstract and detached perception," and "perception at a distance" to make clear the difference, "in a perceptual sense," between a man reading a book about the horrors of a battlefield and the soldier perceiving them "directly" in the battle itself. The present writer "perceives" some advantages in avoiding, in technical literature, the term perception where other cognitive functions, having another name, are meant. In fact, the reader of the book does not "perceive" the horrors. On the other hand, Lindner (46) describes a "perceptual test" in which the subject is presented with several series of five drawings each. The first drawing of each series is only sketched in a very few lines, the four following cards showing progressively more and more of the final representation. The subject is presented with the first very sketchy drawing and is asked "to report to the examiner what he thinks the whole drawing will be." This complex form of imagery and projective guessing is called a "perceptual function." In this way, the differences between various cognitive functions are leveled. A more integrative conception of the behavioral process, and the emphasis laid on personality factors in cognition in general, should not necessarily bring confusion in their train. Experimental research cannot have any profit from such a leveling of processes and a lack of precision in terminology.

A more fruitful development in the relationships between different aspects of cognitive functions is found in studies revealing some functional dependencies between, for instance, perceptual and more elaborate intellectual

processes. New evidence for some "continuity" between both is presented in a stimulating article by Krech & Calvin (36). It is shown that the perception of relatively simple stimulus patterns (horizontally and vertically ordered dots) is to be conceived as a process developing during a time period and proceeding through a hierarchical order from more homogeneous to more differentiated levels of organization. The ease with which a subject progresses in a short time through such a hierarchy of perceptual organization is related to his level of intellectual performance (vocabulary test). With regard to the same general problem of the perceptual and intellectual levels of cognitive processes, important evidence is given by the work of Michotte (55) showing some "prefigurations," on the perceptual level, of the most abstract intellectual concepts such as causality and reality, and by Piaget (60, 61) emphasizing the profound operational differences between perceptual and intellectual functions.

PERSONALITY DEVELOPMENT

Cultural determinants.—Several important studies were published during the year on the influence of child training on personality characteristics. One of them, a contribution by Whiting & Child (82), is inspired by a most promising trend, bringing together the valuable insights of clinical psychology with the experimental findings of behavioral sciences, and testing personality concepts in a cross-cultural setting. Most of the hypotheses in Whiting & Child's *Child Training and Personality* are drawn in fact from clinical psychology, especially psychoanalysis, but findings are reformulated and interpreted in the light of general behavior theory. The whole results in a broader and a more precise definition of experimental and clinical concepts.

The authors have gathered data from 75 societies with regard to child training practices (weaning, toilet training, sex training, independence training, aggression training) and customs relating to illness. The two questions asked are: (a) Do early experiences of satisfaction or socialization anxiety, as developing from severe discipline in the field of child training practices, have a persisting effect on adult personality; (b) Do personality characteristics developing from these cultural practices determine, in turn, any of the beliefs and customs in that society. In a concrete way, it may be that the practice of severe weaning in a given society leads the typical child to develop a motive of anxiety concerning oral activities. In adulthood, this persisting anxiety may manifest itself in typical beliefs and attitudes toward a variety of anxiety inducing situations, such as illnesses, which tend to be attributed in that society to oral activity. Thus, the connection between both cultural customs (severe weaning and responses to illness) is traced through hypothetical personality processes (the anxiety motive and its persistence in adulthood). This interaction is called personality integration of culture (p. 35).

As to the problem of personality development, the only one in which we are here interested, the antecedent variables are various aspects of child

training. They include oral,*anal, sexual, aggressive, and dependence "systems of behavior." The consequent variables are the explanations for illness and the techniques of therapy. These consequent variables are the cultural data used by the authors as indices of personality characteristics of the typical members of a given society. Three judges (graduate students) are presented with adequate ethnographic reports and are carefully instructed in order to make quantitative judgments (e.g., ratings on a seven-point scale and rankings) relating to initial satisfaction, severe discipline, socialization, anxiety, etc., in each of the aspects of child training. On the other hand, customs relating to illness are analyzed and used "as a sort of projective test for a society as a whole" (p. 120). It is assumed that the unrealistic and magical customs which survive in this area are those which "resemble the phantasies to which the members of a society would individually be led by the personality characteristics they have in common" (p. 121). This seems to be the most vulnerable point in the whole construct.

The results confirm some of the authors' psychoanalytic hypotheses or suggest more precise definitions. Fixation is the personality development process which is most carefully studied. Clear confirmation is obtained for the hypothesis that a high degree of frustration of a particular form of behavior produces a continuing fixation of interest on that behavior (negative fixation). Positive fixation, on the contrary, i.e., fixation as a consequence of a high degree of indulgence, is not sufficiently confirmed in this study. In any case, positive and negative fixation seem to be different processes. The evidence for a lasting effect on personality of child training practices is most striking and consistent, in this study, for the oral, the dependence, and the aggressive behavior systems. As to the origins of guilt feelings, data and evidence are more questionable. It is shown, however, that guilt seems not to be entirely explicable as a simple consequence of the degree of anxiety aroused by socialization. Support is given for the psychoanalytic interpretation of guilt as a consequence of the child's identification with the parents. It is also confirmed that the unrealistic fear of others is primarily associated with anxiety about aggression and with the strength of the tendency to be aggressive. The authors are wise in adding, as a conclusion of their very careful study, that further research is needed "before we can feel a great deal of confidence one way or the other in a judgment about the validity of any of the hypotheses we have tested here" (p. 324). It is worth-while to stress the necessity of such methodological precautions in testing the universal value of clinical concepts in a cross-cultural setting.

A study on aggression and dependency as determined by child-rearing practices in an American cultural setting was performed by Sears *et al.* (73). The subjects were 19 pre-school girls and 21 pre-school boys in a middle class population of Middle Western origin. Aggression (threatening, damaging, fighting, rule-breaking, etc.) and dependency (seeking attention, help, approval, nearness, etc.) were measured by direct behavior observation in the preschool, teacher's ratings, and doll play (phantasy aggression and depend-

ency). Child rearing antecedents were gathered from verbal reports by the mothers relating to the severity of infant and current frustration, maternal nurturance, the mother's punitiveness toward the child, etc. "Most of the antecedent-consequent relationships discovered were of relatively low statistical reliability" (73, p. 233). The method used in gathering information about child-rearing antecedents by verbal report of the mother also lacks precision and reliability. Notwithstanding those facts, the authors conclude that "the kind and amount of frustration and punishment experienced by the child are major determinants of the properties of both the dependency and the aggression drives" (73, p. 233). Results are interpreted in a theoretical framework of learning theory.

The influence of family situations on the development of the achievement motive has been shown by McClelland *et al.* (49). The mothers of sons with high achievement motivation represent an "individualistic" family pattern stressing early independent activity. Low achievement scores seem to be related to the protective family syndrome.

The influence of school teachers' personalities on personality characteristics in their pupils is suggested by the findings of Amatora (4). One hundred classroom teachers were rated on a 22 item personality scale by three fellow teachers in the same school. Their pupils (grades four through eight in public and private, rural and city schools) were rated on a corresponding child personality scale by eight classmates and three or four teachers. Positive relationships were found on all of the 22 personality characteristics measured. Most of them were highly significant.

The influence of city life on personality is studied by Hellpach (34). In two well-documented chapters the physical anthropology of big city populations and the ecological characteristics of the city are analyzed. As to the psychological characteristics, the author points to the symptoms of affective leveling, biological impoverishment, linguistic peculiarities, etc., in city populations. The situations analyzed, however, are not to be generalized and seem to be partially outdated by modern urbanization.

Constitutional determinants.—Sex differences have been studied systematically in different researches already mentioned. The biological aspects, however, of these differences are rather neglected. This is the case, for instance, in the previously mentioned study by Sears *et al.* (73). Radical sex differences are found in the processes by which aggression and dependency develop in girls and boys, but they are interpreted simply in function of the differential identifications of male and female children with their mothers. With regard to this rather general attitude in contemporary psychology, as stated by Eysenck in his 1952 review on personality (21), it may be interesting to mention a stimulating and very carefully conducted research on sex and aggressive drive in young pigeons by Valentini (79a). It was found that it is possible to distinguish between male and female subjects, on the basis of objectively defined aggressive behavior patterns, before any external morphological sex character is present (before two months). This points to the

biological factors involved in aggressive behavior patterns and sex differences in this area.

Also with regard to sex differences, it was found by McClelland *et al.* (49) that college women did not show an increase in achievement motive scores as a result of the arousal instructions effective for male college students and based on reference to leadership and intelligence. Higher scores, however, in women were obtained in conditions in which the dimension of "achievement" involved was social acceptability. It seems difficult to decide to what extent this female form of achievement motivation is eventually dependent on biological determinants. The differentiation of sex role concerning striving for achievement is manifested also in the fact that, in our culture, male and female subjects tend to express their achievement motivation in imaginative stories in which the central character is a male (80).

The influence of constitutional factors in the development of personality conflicts is emphasized in a study on neurosis as a maturation problem by Kretschmer (37). More specially, physiological abnormalities in the sexual maturation process were studied in their relation to neurotic and other minor personality disturbances. A group of 50 female nonmarried neurotic patients were examined. In 43 cases there were insufficiencies in the development of secondary sex characteristics, in 40 hypoplasia of the uterus was found, etc.

Elsässer (19) studied the descendants of 38 psychotic couples (schizophrenics and manic-depressive psychotics). Always more than 50 per cent, and in some cases up to 85 per cent, of the lineal descendants were normal personalities. As to the hereditary influence, psychotic descendants always belong to the same psychiatric category as their parents in the case where both parents are suffering from the same type of psychosis.

A very precise study of the expressive factors in the human face was published by Lange (40). No characterological interpretation is ventured, but anatomical and morphological descriptions of muscle contractions are related to specific affective expressions. The respective influence of inborn and acquired characteristics is carefully studied. Very demonstrative illustrations are added.

TEXTBOOKS

Only a very few representative textbooks will be mentioned here. Nuttall (56) published an eclectic introductory manual covering the essential problems in the whole field. Its main purpose is that of "direction-finding" in the field of personality; it is by no means a catalogue or résumé of research findings. The exposé is nonsystematic. Eysenck's new book (22), on the contrary, is a remarkable synthesis of research findings in one specific area. A clear account is given of a large number of studies using ratings, questionnaires and inventories, objective behavior tests, and physiological measures relating to the structure of human personality. Different theories (including typology) are discussed. About his well-known personal views and his subjective interpretation of the findings, Eysenck modestly writes: "The reader

is at liberty to disregard these views completely, and to evaluate the evidence from his own point of view" (p. 317).

Representative for the opposite extreme in the field of personality study is a book by Thomae (78). Personality structures and orientations are "described" here as a developmental process (*ein fortschreitendes Geschehen*) in a rather intuitive and phenomenological way. Although an impressive background of scientific, historical, and literary erudition is displayed, theories are built up without any direct experimental or clinical basis. The work is representative of some trends in postwar German psychology. Metzger (54) has stressed several shortcomings of the book from a more scientific psychological point of view.

The "dynamic" standpoint is represented in a textbook by Sappenfield (71) in which the different psychoanalytic mechanisms of adjustment are described and organized in the framework of an organismic conception of behavior.

CONCLUSION

The present writer conceives of personality as a specific way of behaving and functioning which is characterized by awareness of a life-situation or "world." While admitting that the behaving person may be considered a part of the psychological field (Lewin), one should not forget that, more primarily perhaps, the psychological field itself is an essential "part" or component of personality. Nothing corresponds less to reality than to conceive of personality as an enclosed structure of "empty" and "blind" functions. Therefore, studies on the cognitive or perceptual "openness" of man on his world have been emphasized in this review. On the other hand, specifically human motivation is related precisely to the fact that man lives in a meaningful situation. Integrating and actualizing himself in his world seem to be the essential and most general orientations of his normal behavior. At the beginning, scientific psychology became possible by avoiding these complex problems. Today, its further development is partially conditioned by the way it will face them.

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SOCIAL PSYCHOLOGY AND GROUP PROCESSES¹

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The number of problem areas within Social Psychology is very large and the space allocated to this review is quite limited. This is by way of saying that this review is highly selective. I ask tolerance toward my inclusion of things which others might not include, my exclusion of material which others would include, and any things I may have overlooked. The alternative to such selectivity would have been to give up the opportunity to discuss, at length, things which I would like to dwell on. Having said this, I would like to conserve space by eliminating further introductory remarks.

METHODOLOGICAL CONTRIBUTIONS

In any empirical science there is always a large component of "art" in the conduct of research. To the extent that this "artistic" component is present one learns techniques of research by trying things out and by a sort of apprentice relationship with those who have acquired skill at one or another thing. There should be, and there is, in social psychology a tendency to codify and make generalizations about research techniques.

In the past year three books on methods relevant to social psychology have appeared. One of these, by Hansen, Hurwitz & Madow (48), is a two volume work dealing comprehensively with the theory and practical techniques of sampling. While the book is mainly statistical, and of a highly technical variety, its existence is worth noting here because of the importance of sampling for public opinion surveys.

Another book by Remmers (105) deals with methods of measuring opinions and attitudes. It also summarizes many of the major contexts in which such methods have found application. It is written at the undergraduate level.

A third book, edited by Festinger & Katz (34), is an attempt to provide material on research methods in social psychology which can be of help to the research worker. This book is, in part, a "cookbook." I do not mean this term in any derogatory sense; a cookbook is needed for research techniques. If you want to know how to do a laboratory experiment with groups, a public opinion survey, how to measure or manipulate a given variable, and the like, it would be convenient to have a "cookbook" which told you how to do it.

This book doesn't accomplish that, but it tries to do it and should be quite useful. Each chapter in the book is written by a person who has had considerable experience using the particular method which he is discussing.

¹ This survey of the literature pertaining to this review was completed in April, 1954.

The result is a series of "how-to-do-it" chapters which attempt to convey methodological knowledge, mainly by example, together with attempts at generalizing from the examples. There are still many vaguenesses, lacks of knowledge about how to do things, lacks of information about the relative desirability of different procedures and the like. But there is also an impressive array of knowledge. There is useful information on how to do surveys, field studies, experiments in natural and laboratory situations, interviewing, observation, sampling, and others.

There has been considerable work done during this past year in an attempt to further our knowledge of techniques of research and a good statement by Vinacke (138) of some unsolved methodological problems. Progress in this area of advancing methodology is painfully slow for a variety of reasons: some of the methodological research is poorly conceived, some is well conceived but turns out not to yield any new information. Let us look into this more closely.

One basic methodological necessity is, of course, the development of measuring instruments. This type of thing fits in with a long tradition of test construction in American psychology and continues to be a popular pastime. There have been published quite a large number of articles which we shall not discuss in detail and shall not list which have the following format. A questionnaire or test is developed and administered to a group of people, usually a large number of cases. The author of the article reports that the test has a certain coefficient of reproducibility or a certain reliability. Sometimes it is left at this; sometimes the author also shows that the test score is consistent with remarks the persons made while taking the test or shows a moderate correlation with some other similar test. In these articles there is no indication that the test is measuring anything important or how important it is or what it is important for. There seems to be the conviction that if it is highly reliable, that automatically makes it worth while.

This is not enough. When we say that we need measuring instruments we mean that we need instruments for measuring variables that are important whether theoretically or practically. The demonstration of importance is a demonstration that this variable is related to things which it is supposed to explain or predict or result from. Peak, incidentally, has an excellent discussion of this very point in her chapter in the book mentioned above (34). I will discuss one pair of studies in detail to show one of the consequences of this uninhibited construction of new measuring instruments.

Adelson (1) reports the construction of a special test to measure authoritarianism in Jews. Commenting on the importance and success of the California F scale (2) he makes the point that it would be important to have a good scale for measuring the same thing on members of a minority group. With the help of careful preliminary interviews he constructs a test which he then administers to Jewish students. He finds that his test correlates with the California F scale ($r = +.67$). This, in addition to his impressions and qualitative judgments, leads him to feel it is a good measuring instrument, which it

very well may be. There is, however, another study reported by Radke-Yarrow & Lande (104) which is concerned with essentially the same problem. They also want to investigate authoritarianism among minority group members and, luckily for the comparison we want to make, also choose to use Jewish subjects. They, however, simply administered the California F scale together with questions to measure anti-Semitism, Jewish ethnocentrism, and degree of submission to the majority group. They report correlations of the F scale with the other three variables of $+.58$, $+.73$, and $+.74$ respectively. In other words, it looks as though the original California F scale is an excellent instrument for measuring authoritarianism in Jews. We may then well ask what the value was in constructing a new scale.

Perhaps in these days when space in our journals is at a premium and the lag between receipt of a manuscript and publication is very great indeed, journal editors should encourage the constructors of new tests to submit their articles after they have done additional research to show the importance of the new test. They might also have one page of listings headed "New Tests Available."

There is a very excellent methodological study by McGinnis (91) which also highlights the inadequacy of simply determining the reliability or reproducibility of a measurement scale. It also bears on the problem of questionnaires versus open-ended interviewing. One hundred male university students filled out a fixed alternative questionnaire concerning attitudes toward marriage. Each one was also interviewed using exactly the same questions but allowing open-ended responses. Half the subjects were interviewed a week before taking the questionnaire and the other half were interviewed a week after taking the questionnaire. Each interview was recorded and the answers to each question were coded in the same categories as the fixed alternatives on the questionnaire. The results were tested for scalability separately for the questionnaire and for the interviews. The questionnaire responses yielded a scale which had a reproducibility of $.93$; the interview data yielded scales with reproducibility of $.95$ with one coder and $.96$ with another coder. The two scale scores for the different coders based on the interview correlated $+.97$ for the 100 subjects. In other words, both the questionnaire and the interview yielded good "unidimensional" scales, and in addition there is good evidence for very high coder reliability on the interviews. But what about the correlation between the scale based on the interview and the scale based on the questionnaire? This correlation turns out to be only about $+.35$. In short, they are measuring largely different things. In other words, the same questions asked in two different ways yield highly reproducible scales that hardly relate to one another.

A study reported by Libo (76) is an example of good methodological research although its results emphasize the weaknesses rather than the strengths in the instruments with which he deals. He sets out to evaluate existing methods of measurement and experimental manipulation of "group cohesiveness" or "attraction to a group" and also to construct a new means

of measuring this variable. His interest in "attraction to a group" stems from the fact that a number of studies [see Chapters 7 and 11 in the book edited by Cartwright & Zander (19) for a review of these studies] have shown that this variable is related to many aspects of the influence process in groups. In addition there has been some interchange in the literature: Gross & Martin (44) attacking the concept as being vague and Albert (3) and Schachter (115) defending the concept in terms of its empirical value. All have stressed, however, that more methodological research on the measurement and manipulation of "cohesiveness" is needed. Libo sets up experimental groups in which he manipulates the attraction to the group in the way in which previous experimenters had done. He then measures the attraction to the group by means of a questionnaire (which is what has most frequently been used in other studies) and also by means of a technique of having the members of the group write stories about pictures. These stories are then coded in terms of whether or not they reveal attraction to the group. This measurement device was patterned after the work of McClelland and his colleagues (88).

In addition to these measurements, Libo devised a behavioral criterion against which to validate the manipulations and the measurements. Reasoning that the stronger the attraction to the group the greater would be the desire to remain in it and keep the group going, he created a situation where each subject, out of view of anyone else, goes into one room if he wants to continue with the group and attend future meetings, and goes into a different room if he wants to leave. He finds that the experimental manipulations and both measurement devices have some validity. Both measures discriminate significantly between experimentally manipulated high and low attraction and also differentiate significantly between those who remain with the group and those who leave. Both the questionnaire and the projective instrument are equally good in relating to the experimental manipulation and staying or leaving. The correlation between these two measures, however, is only $+.35$. The relationships among the two measures, the manipulations, and the behavioral criterion are all significant but relatively low. While we must conclude that there is validity in the measures and the manipulations, it is equally clear that there is very much room for refinement and improvement.

Another useful study is that reported by Blake & Brehm (10) in which they show that completely recorded instructions carried through the whole session can be effectively used in experiments with groups.

One area of methodological problems which continues to be studied centers around various dilemmas and difficulties associated with public opinion surveys. Each such study usually attacks a very specific and concrete problem and, usually, does not yield a great deal of information. It is difficult to see, however, how one study could yield a great deal of information in this semiscientific, semiartistic realm. The accumulation of these little bits and pieces will, however, improve our knowledge of how to do research. I will

briefly mention some of the problems which have been attacked in the last year together with the findings.

Star (127) raises the question as to whether the opinion of a household can be adequately ascertained by interviewing one adult in the family. Data comparing male and female responses on a survey regarding household opinions about subscription television lead to the conclusion that, at least on a topic such as this, it can be done. Longworth (79) reports a systematic study of factors affecting the per cent returns on a mail questionnaire. Varying things such as inclusion of a newspaper article, typed rather than mimeographed letter, and others succeed in raising the per cent return from an initial 14 per cent to 26 per cent. A follow-up phone call one week after the letter is sent out, however, succeeds in raising the return to 63 per cent. Metzner & Mann (94) find no consistent differences in responses to a questionnaire when the questions are grouped differently. Gross & Mason (46) discuss some of their experiences in conducting interviews lasting about 8 hr. Spiegelman, Terwilliger & Fearing (125) find that allowing discussion among coders about category definitions did not improve their reliability in categorizing comic strips. Freeman (38) finds that there is considerable error in respondents' statements of whether or not they voted when these statements are checked against actual records. He finds, however, that good prediction of who votes is possible from statements of intention to vote and whether or not the person is registered.

Before leaving the topic of methodology there is one study I want to mention in some detail because it is novel and interesting. Kuhn & McPartland (72) asked subjects to list 20 items in answer to the question, "Who are you?" They distinguish between "consensual" statements and "subconsensual" statements. The former are ones which are overt designations such as "I am a boy," "I am married," etc. The latter are covert designations such as "I am happy," "I am an interesting person," and the like. They report a very curious finding, namely, that persons list consensual statements before they list any subconsensual statements and, after having listed one subconsensual statement, practically never list any more consensual statements. Analyzed as a Guttman scale it yields a reproducibility of over .90. The authors report that there is some tendency for members of minority religious groups to give a larger number of consensual statements than members of majority religious groups. In the absence of more data it is difficult to know what this measure means or implies, and it is too bad the authors did not conduct more research using this instrument. The finding that the responses order themselves as they do is, however, provocative and worth noting.

SUBSTANTIVE REVIEWS

This past year must not have been much of an occasion for taking inventory in the area of social psychology. The number of review articles, collections of readings, and textbooks is unusually small. Those which have appeared do not add much to our knowledge or perspective although they will

probably be quite useful pedagogically. Roseborough (112) reviews the extant literature where small groups have been studied experimentally. Faris (31) discusses and traces the history of the development of interest in the small group and stresses the importance of studying the primary group. This discussion is entirely from the point of view of the sociologist and social theorist.

Cartwright & Zander (19) have edited a book which presents together a number of articles in group dynamics most of which have been published. The selection of articles is very excellent. The editors also do a superb job of writing chapters which introduce each section of the book. These introductory chapters put some of the problems in good perspective. They also make an heroic effort to supply integration in the various areas but this effort is only partially successful. There are not enough data, not enough agreement on problems and variables, and not enough good theory to enable such integration to take place.

Another book of readings presenting a large collection of previously published articles dealing with public opinion, attitude formation, and mass media research has been edited by Katz *et al.* (64). It presents an excellent selection of papers ranging from the writings of political scientists to carefully controlled laboratory experimentation. It is impressive to note how many advances in understanding have been made in recent years. The collection should prove useful in the classroom.

We have devoted so little space to these reviews and collections not because we do not consider them worth while, but rather because they summarize what has been done in the past rather than make any new contribution.

PERSONAL CHARACTERISTICS AND SOCIAL BEHAVIOR

Social psychologists have been talking for a long time about the importance of studying the relationship between personality, social attitudes, and social behavior. This is, indeed, an important area for study. Not very much actual research has been done in the past, however. One of the reasons for this, probably, is the lack of well defined personality variables which have clear social implications. The work of the California group on the authoritarian personality (2), by providing a theoretical framework and a measuring instrument of demonstrated effectiveness which did have clear social relevance, has done much to stimulate research in this area. In the past year a number of studies have appeared relating measures on the California F scale to social attitudes or to social behavior.

Janowitz & Marvick (62) report on data collected in a public opinion survey which included six questions measuring authoritarianism. They find that higher authoritarianism is associated with higher age, lower education, and lower socioeconomic class. They also find that high authoritarians more frequently have "isolationist" views on foreign policy and vote less often in elections. Davidson & Kruglov (29, 71), administering the F scale to college

students, find that those low on authoritarianism are somewhat older and farther along in college than those high on authoritarianism. They also find that those who are willing to be interviewed later on are lower in authoritarianism than those not willing to be interviewed. Those willing to be interviewed are also somewhat older and farther along in college.

What do results such as these mean? First of all, they add a few more things to the rapidly growing list of measures which are found to correlate with the F scale. But, more important, they seem to me to raise some problems about the interpretation of the F scale itself. The F scale has been frequently talked about as if it measured some relatively deep and enduring aspects of personality. But here we find that the experience of going to college lowers authoritarianism. We also know that the experience of going to a liberal arts college increases liberalism. Mull & Sheldon (98) review a study (17) which showed that in 1941 seniors in a liberal arts college were more liberal than freshmen. They report the results of a repeat of this study in the same college conducted in 1951 and find the same result. This type of finding has also been reported by many others. It seems possible, then, that the F scale is relatively easily affected by environmental experiences and perhaps it correlates with political-economic conservatism because the same factors tend to change both of them.

The finding that in the general population increased age is associated with higher authoritarianism can be viewed in the same way. Many surveys and polls have reported more conservative attitudes among older persons. Toch (136) reports a study to clarify the interpretation of such findings. He states two alternative explanations of this increased conservatism with age: it could be a psychological change that accompanies growing old or it could be a result of cultural differences during the growing up period, older people have grown up during a different era than younger people. He examines data from surveys conducted by the Office of Public Opinion Research over the last decade and finds that the cultural differences during the growing up period can be ruled out as an explanation. If we accept this then it seems that growing older seems to be simultaneously affecting the F scale and conservatism.

Prothro & Melikian (102) also report a study which raises problems about the meaning of the F scale. Reasoning that F scale scores should be higher among persons who grow up in an authoritarian culture, they administered the F scale and the political-economic conservatism (P-E-C) scale to 130 persons who grew up in the Near East and were attending the University of Beirut. They made a convincing case for regarding the Near East countries as having a more authoritarian culture than the United States within the meaning of the California studies. They do, indeed, find that these Near East students score higher on the F scale than students in the United States. However, they find no correlation among these Near East students between the F scale and the P-E-C scale. This raises the possibility that the F scale, administered to persons in a "nonauthoritarian" culture, may measure some-

thing quite different than when administered in an "authoritarian" culture.

Christie (21) in reviewing work on the F scale shows that the score on "authoritarianism" seems to be affected by intelligence, influence processes in groups, education, and such factors. He states that "These findings are interpreted as running counter to the general argument in *The Authoritarian Personality* which emphasized purely personality determinants of potential fascism and ethnocentrism and discounted contemporary social influences" (21, p. 194).

Other research reported the last year relevant to the F scale includes a study by Mahler (82) which shows that the F scale, the E scale, and the P-E-C scale all correlate with attitudes towards socialized medicine, a study by Mishler (96) which shows that the F scale and the E scale do not correlate with a test of how people choose between actions where ideology and "making an exception for a friend" are in conflict, and a study by Sullivan & Adelson (133) who show that ethnocentrism is related to general misanthropy.

Closely connected with the work on authoritarianism is the research by Rokeach (109) which has shown that mental rigidity is related to ethnocentrism and to social attitudes. A study by Goodstein (43) using three different tests of mental rigidity finds no correlation among the three and, furthermore, finds no correlation of any of them with a variety of social attitudes. Such failures of replication should be given some attention. In the present instance it probably means that there is need for more precisely defining what is meant by mental rigidity.

A number of other studies which attempt to tackle the problem of the relationship between personal characteristics and social behavior or social attitudes are definitely worth commenting on. Some of these produce findings which are merely suggestive but, in an area where progress has been so difficult, good suggestions may be exactly what is needed.

Hoffman (53) reports an extremely interesting study of personality factors affecting "compulsive conformity." The experimental situation in which he measures compulsive conformity is well chosen for his problem. Groups of people make judgments of the length of a stimulus figure, each judgment being made privately. The experimenter then reports an almost absurdly large value as representing the average of the group. They are then asked to once more make private judgments of the length. Those who change their judgments toward the large reported group average are selected as "compulsive conformers" on the reasoning that, with the reported average so out of line, and with the actual judgments being made privately, "group-induced changes in judgment reflect irrational needs to conform" (53, p. 384). Those who did not change their judgments are selected as nonconformers. The Thematic Apperception Test, a sentence completion test, and a questionnaire were administered to all subjects. The author finds that the compulsive conformers have less ego-strength, stricter morality, more acceptance of authority, and a more conventional belief in God. The results are explained in terms of a largely psychoanalytic theory which attributes the compulsive conform-

ity, weak ego-strength, and the other factors to early childhood experiences with parents who are punitive, strict, and coercive. The studies reported by Hovland, Janis & Kelley (57) on personality and susceptibility to persuasion (Chapter 6 of their book) lend rough support to these results. These studies show that shyness, feelings of social inadequacy, low self-esteem, and inhibition of aggression tend to be more characteristic of easily persuaded persons than of persons difficult to persuade. There seems to be a congruency between all these results but, with the vague designations of personality characteristics that are used, it is impossible to really say one way or another. There is a great need for clarifying and specifying these personality designations and for further research thereon.

Riggs (107) proposes three new personality variables together with some suggestive evidence that they may be related to social behavior. Persons were given a "sentence forming" test and, from the responses, three categories were scored: literal (showing an orientation toward factual, practical things), indirect (showing an orientation toward inner, abstract things), and exciting (showing an orientation toward power, sex, etc.). Evidence is presented to show that, on a sociometric test, literal people tend to choose others also high in literal responses and that those high on exciting responses choose others high on the same thing. Some weak, but suggestive, evidence is presented that persons high in indirect responses do not get elected to office. While the obtained results are only tentative, this study deserves emphasis because it is highly likely that new ways of conceptualizing and defining personality variables are necessary before very much progress will be made in relating them to social behavior.

Another highly suggestive study is reported by Cox (28) who investigates the effect of play therapy, with children living in an institution, on their sociometric status. The older children who received play therapy were the only ones who showed an increase in sociometric status and maintained this increase over a period of 27 weeks. The author reasons that the older children were the only ones who had sufficient control over their social environment to be able to carry over the benefits derived from the therapy.

A few studies have been reported which attempt to relate personal characteristics of a sort which would not generally be called "personality factors" to social behavior. Unfortunately, none of them seems to be very noteworthy. Martin & Siegel (85) construct a test of biographical items which they relate to a "group participation scale." The score on the biographical items test is found to correlate $+ .39$ with the "group participation scale" on 24 cases not used in the original construction of the test. This is a relatively low correlation with rather few cases. What is even more bothersome, however, is an examination of the original scale of "group participation" which was constructed by Pepinsky, Siegel & Van Atta (101). One discovers that this test is based entirely on sociometric responses and that no evidence at all is presented concerning its validity or usefulness. Smith, Sluckin & Graham (122) investigate the effect on the behavior of groups of children of different de-

degrees of ability and different degrees of assertiveness in the children making up the groups. The groups were required to work on block building and block sorting tasks. The authors find that the higher the ability of the children making up the groups the greater was the degree of friendliness shown, the greater the cooperation, and the better the quality of the work. Assertiveness, as rated by the headmistress of the school, was not found to relate to any of these measures.

Burgess (14) reports that among old people (average age over 75) living in an institution, higher sociometric status was related to greater happiness and feelings of greater usefulness.

PUBLIC OPINION AND MASS MEDIA

Public opinion, attitudes on important issues of the day, the effects of mass media, and other similar topics continue to occupy the interest of a relatively large number of investigators. There continues, however, to be a lack of organization to this field. Somehow studies do not add up and attempts at theoretical integrations or even at providing conceptual frameworks do not seem to work out. Fearing (32), for example, attempts to provide a framework for thinking about the impact of mass media by distinguishing the communicator, the recipient, the content, and the situation. He states that these must be considered as interdependent and suggests variables such as specificity of intent, reality, and authenticity as being important to study. It is difficult to see where it leads, however, and an empirical study of the content of comic books in these terms (126) doesn't seem to add much.

Foote & Hart (37) also attempt to provide a framework for dealing with public opinion by analyzing the stages of its development. These stages, as they see it, go something like this: first there is a problem defined for the public, then the perception that something must be done about it followed by a period of discussion and comparison eventuating in reaching a decision and doing something. They feel that only in the last two stages can one really talk about "public opinion." If this analysis is correct there are implications, which they point out, for the timing and content of public opinion surveys. It remains to be seen, of course, whether this schema will help in guiding research and in organizing research results.

One of the attempts to deal more effectively with problems of public opinion and mass media, which has seemed promising to this reviewer, has not received much attention this past year. In the past 10 years there have been several investigations that have suggested that group relations, group memberships, and reference groups have a great deal of importance in determining and mediating the effects of mass media. Riley & Flowerman (108) presented some stimulating ideas in this connection. Friedson (39), starting from their approach, reports an investigation in which he asks whether the social situation in which the person is exposed to the particular mass media has any effect on how they react to it. This is a good question, but, unfortunately, the study actually never gets near it. All he reports is that children tend to

read comic books alone, they tend to go to movies with their peers, and tend to watch television together with their family.

A number of studies which have appeared this last year carry an emphasis on the consistency among opinions and among attitudes. This could well prove to be a very fruitful line of approach to these problems. If progress were made concerning which groups of opinions tend to be consistent with one another, which tend to be relatively independent of one another and the implications of such consistency clusters for change of opinions and attitudes, much light would have been shed on many problems. The studies I will mention below have made some progress in this direction.

Suchman, Goldsen & Williams (131) show a high degree of consistency among students' attitudes toward the United Nations, the Korean War, and present conflict in the world. In another article (132) these same authors show, however, that students' attitudes toward impending military service is only slightly related to this attitudinal cluster but is related strongly to how much military service would disrupt their own personal plans. Thus we see a cluster of items among which there is high consistency whereas some others, which one might have expected to be related, are relatively independent of them. In a further analysis of the attitude cluster which does show consistency, Blau (11) compares attitudes in 1950 with those in 1952 on the same students. He finds that changes in attitudes which occur within this cluster are overwhelmingly in the direction of increasing the consistency within the cluster. He attributes these changes toward increased consistency to the pressures of social groups to which the person belongs on the assumption that inconsistencies in his own cluster will be accompanied by disagreement with his friends, resulting in an influence process which will change those attitudes of his which were inconsistent. He also shows that social isolates and those who report difficulty making friends are less likely to have changed their attitudes over the two-year period. This, of course, is corroboration for the interpretation that change is related to pressure in social groups. The explanation of why this change results in greater consistency is still open to question, however.

Diggory (30) also reports an interesting study of consistency among attitudes. He measured attitudes toward church, religion, war, birth control, etc. Comparing the data for males and females he finds that the only difference in means exists for attitudes toward church and religion where the females are more favorable than the males. He finds, however, that there are different patterns of consistency among males than among females. In general, for the females there is more consistency among those attitudes that correlate with attitudes toward church and religion. It may well be that church and religion are more important for females on the average and hence more pressure for consistency among the relevant cluster of attitudes.

Other relevant studies here include that of Scott (116) who shows consistency among various opinions concerning civil defense and one by Showel (121) which shows consistency among various indications of political awareness.

Two other studies, because of the way they fit together and raise interesting questions, deserve some detailed mention here. Henry & Borgatta (52) administered a questionnaire to officers and enlisted men in the air force to obtain their opinions concerning the proper punishment for an enlisted man who had been found guilty of desertion. The results show clearly that, no matter what the circumstances of the deserter are described to be, the enlisted men recommend harsher punishment than do the officers. This, at first glance, seems to be a curious result in that, on many grounds, one might expect enlisted men to feel more sympathetic to someone in their own group. On second thought, however, there seems to be some casual corroboration of this finding. For example, any teacher who has asked students to grade one another knows that most often they grade each other much lower than the instructor would. Students on disciplinary committees are frequently reported to be much sterner than the faculty on these same committees when judging student offenses. It may be that this result comes about because of a tendency for those in the same role as the transgressor to resent these transgressions more or perhaps a tendency to see only black and white on the part of persons unaccustomed to the situation of passing judgment on others.

Before we make up our minds about it, however, let us look at the second study. Hartung (49) administered a questionnaire to a sample of persons in the wholesale meat industry and also to a sample of the general public. The questionnaire described five cases of criminal and five cases of civil violation of the OPA (Office of Price Administration) laws by wholesale meat dealers and asked each respondent to rate the seriousness of the offense. Naively generalizing from the above study on officers and enlisted men, we might expect the meat dealers to be more harsh than the general public. But here we find the reverse to be true. The meat dealers are somewhat less harsh on the criminal cases and much less on the civil cases than the general public. Some of the factors that might account for this apparent contradiction are the absence of direct contact between wholesale meat dealers and the public, the absence of a status relation between them, and also perhaps the prevalence of OPA violations as compared to the degree to which desertion is practiced. It seems to me that important problems of theory and fact are raised here.

SOCIAL INFLUENCE PROCESSES

This problem area, namely, the social determinants of opinion and attitude change, has attracted considerable attention in recent years, both in terms of experimental work and theoretical writing. The work that has been done can be said to have attacked the problem from three major angles: the effects of membership groups and interaction among members, the effects of nonmembership reference groups and the effects of public or mass communications. There has been little success in tying these three aspects together, although there is a growing awareness that this must be done. Sarnoff & Katz (113), for example, examine various types of theories concerning the bases for attitude change which emerge largely from a considera-

tion of different ones of these three aspects of the problem. Their principal thesis is that all of these bases for attitude change operate, sometimes alone, sometimes in combination, and that their effectiveness should depend upon various aspects of the personality of the individual. They outline a proposed program of research to deal with all of this at once. If their program of research is successful it may go a long way toward integrating the work on these problems.

By all odds, the major contribution of the year in this area is the series of studies reported by Hovland, Janis & Kelley (57). These studies are almost all very impressive, not only for the excellent way in which they are executed, but mostly for their success in pulling out meaningful problems and demonstrating neatly how various factors work. By and large, however, they tend to ignore the group and interaction variables in their designs and in their interpretations. Sometimes this leads into what seem, to me, to be ambiguities. Let us take some of the work on the trustworthiness of the communicator as our first example. Hovland & Weiss (59) report a study which shows that where the source of a communication is seen as trustworthy there is more change of opinion in the direction advocated by the communication than where the source is seen as untrustworthy. They also show that after several weeks have gone by some of those who heard the communication from the trustworthy source have changed back and some of those who heard it from the untrustworthy source have continued to change even more in the direction advocated by the communication so that the difference between the two conditions disappears. They call this a "sleeping effect" and attribute it to remembering the content of the communication while forgetting the source. To check this interpretation Kelman & Hovland (68) did another experiment in which some of the subjects were reminded about the source of the communication prior to the delayed measurement of their opinions. When this is done the "sleeping effect" all but disappears.

But let us examine the procedure of the experiments more closely. In order to have adequate controls, one-fourth of the subjects in each class read a particular communication attributed to an untrustworthy source; another fourth read the same communication attributed to a trustworthy source; another fourth read a communication advocating exactly the opposite attributed to an untrustworthy source; while the last fourth read this opposite communication attributed to a trustworthy source. There then elapsed three to four weeks before the delayed measurement of opinion during which time we may imagine that there was a quite a bit of interaction among these members of the same class. We can also imagine that the experimental procedure resulted in some discussion among them concerning the issues on which their opinions had been measured. The differences of opinion among them which had been created by the well-balanced experimental design would lead to an influence process which would tend to move them back to agreement. This kind of influence process has been shown in a number of studies and experiments. The "sleeping effect" might then simply be attributable to the operation of

this social influence among the members of the class. The fact that reminding them of the trustworthiness of the original communication source recreates the difference still does not remove the ambiguity of interpretation. My own hunch is that if, instead of the design they used, one whole class read something attributed to an untrustworthy source, and a different class, the members of which did not have contact with the first class, read the same thing attributed to a trustworthy source, the "sleeping effect" might not manifest itself.

Let us take, as another example of the same kind of ambiguity, the experiments on the relative effectiveness of one-sided versus two-sided communications. Hovland, Lumsdaine & Sheffield (58) showed that a one-sided argument, that is, a persuasive communication which only presents the arguments on one side of the question, seemed to be more effective in producing opinion change in the direction advocated for those persons who were initially favorable to that point of view. Those who were initially opposed, however, were more influenced by a persuasive communication that also mentioned the opposing arguments. Over all subjects, there was no difference in the effectiveness of the two types of communications.

Lumsdaine & Janis (80) present a further study in which they investigate whether or not there is a difference, resulting from those two types of persuasive communication, in how resistant the persons are to counterpropaganda. A week after having heard the original communication, half of the subjects were exposed to a counterargument before asking them to state their opinions on the issue. The results show very clearly that the counterargument is almost completely effective for those who originally heard the one-sided presentation but is almost completely ineffective for those who originally heard the two-sided presentation. This result is interpreted in terms of the direct effect on the person of the two-sided argument in already preparing him to meet counterarguments. This may be correct. It also seems possible that the two-sided presentation, by stressing the controversial aspect of the problem, might well set off considerable discussion among the subjects after hearing it, while the one-sided presentation might not stimulate such interaction among them. We may, then, actually be comparing the resistance to counterarguments of one condition where the opinion is firmly anchored in a membership group with another condition where the opinions are not so anchored. If this were true, while it certainly does not vitiate the importance of the findings, it does change the interpretation and raises new problems. In the light of our knowledge concerning the effect of interpersonal and group influence processes it does seem necessary to consider them as possible mediating effects in these experiments.

I do not mean to imply that the authors of this book are not aware of the importance of these interactions among persons. Indeed, one chapter of the book deals with an excellent series of studies on exactly this problem. One of these has been published separately by Kelley & Volkart (66). The others are as yet unpublished except for their descriptions in this book. These stud-

ies show clearly that the degree to which an opinion is anchored in a group directly affects its resistance to a counterargument.

The experiments reported in this same volume on the effects of fear arousing communications, the effects of preparatory communications on subsequent behavior, and the effects of active participation on opinion change could also be discussed at great length were it not for space limitations. Since most of the experiments on the first two topics mentioned have been reviewed in previous years, I will skip over them. The experiments by Janis & King (61) and the experiment by Kelman (67) are exceptionally interesting demonstrations of the fact that if a person is somehow put in the position where he advocates verbally a given opinion on an issue, and tries to do a good job of his presentation, he will tend to change his opinion so that it coincides more with what he had been saying.

Before I leave this series of studies I would like to emphasize their excellence once more and to stress the fact that they are the first concerted series of studies to begin to shed appreciable light on problems of opinion and attitude change resulting from direct exposure of the individual to public or mass communications. If we compare these studies with the more usual type of study of attitude change such as one which appeared this last year by Barkley (4) the difference in approach and in promise becomes clear. In this last mentioned study it is shown that students' attitudes became slightly more pacifist from 1935 to 1937, changed slightly away from pacifism by 1939, continued to change progressively toward more and more favorable attitudes toward war to 1943 and have been relatively stable since. Such a study, of course, provides a description of what has taken place but makes no progress toward an understanding of the dynamics of opinion and attitude change. The studies summarized by Hovland, Janis & Kelley do increase our understanding of these dynamics and pin down some relevant variables.

Since these studies represent mostly what might be called "laboratory investigations" it is interesting to speculate briefly on some of the problems of applying such results to so called "real life" situations. It seems that one major difficulty lies in the fact that in these experiments the subjects were, in essence, forced to listen to or to read the persuasive communication that was being investigated while in real life situations there is an overwhelmingly important selection in who exposes himself to what. The possibilities of application, then, would be enormously increased if we also had good knowledge about the determinants of exposure to communications in free situations and some knowledge about the types of situations in which self-selection with respect to exposure does not operate.

Lipset (78) reports an interesting study which bears on this. He collected data from students at the University of California during the recent controversy over the signing of noncommunist oaths by faculty members. Data were obtained concerning opinions as to whether or not communists should be allowed to teach, opinion as to the oath itself, general political liberalism, and information as to which newspapers they read. Liberals were found to be

more often in favor of letting communists teach and more often opposed to the loyalty oath. Liberals also tended to read liberal newspapers while conservatives tended to read the more conservative newspapers. All of the papers in the area, however, were editorially opposed to letting communists teach but the more liberal papers were opposed to the loyalty oath. In this situation it was possible to analyze the data, holding liberalism-conservatism constant, to show that students' attitudes toward the oath tended to agree with the opinions of the newspaper they read. Since liberalism-conservatism was probably the major selective factor in determining which paper they read, the author is able to make a convincing case for concluding that the newspapers did influence the opinions of the students. This type of situation, where the self-selection factors were not overwhelming, could be used to test the conclusions from laboratory studies.

Opinion and attitude change, as it is affected by interaction among persons and within groups, is the subject of two theoretical papers and a number of experimental reports. Newcomb (99) presents a theoretical framework within which he deals with influence, interpersonal communication and role. This paper is a serious attempt to deal with the details of communication and influence, taking account of the relations that exist among persons, the perceptions they have of each others' opinions on specific matters, and their roles in the group. It is an especially interesting attempt because the whole discussion centers around factors in the individual and relations between pairs of persons. There is almost a complete lack of mention of "group variables" as such. This paper is partly programmatic in that it is intended to serve as a guide for research currently under way. The other theoretical paper is one by Festinger (33) in which the distinction is elaborated between what one might refer to as "real change" in opinion or attitude and publicly expressed change without private change. The former is the type of influence which is the subject of Newcomb's paper. The hypothesis is put forward that the latter, namely, public compliance without private acceptance, occurs under conditions of threat of punishment for noncompliance when the individual is restrained from leaving the situation. Data are needed which are specifically relevant before either of the above two theoretical treatments can be properly evaluated. They are both, however, stated in testable terms.

The experimental papers that have appeared on these problems are rather straightforward, and we can briefly summarize their contribution to our knowledge. Mausner (86, 87) reports two studies of influence on opinions in pairs. Using judgments of length of lines he shows that subjects who were previously given the impression that their judgments were correct are considerably less influenced by the judgments of a partner than are those who were previously given the impression that their judgments were incorrect. In the other study, using aesthetic preference judgments, he shows that when the partner in the situation is seen as an artist the subject is more influenced by him than when he is seen as a fellow student. A study by McKeachie (92) shows that changes in perceived group norms affect individuals' atti-

tudes toward greater congruence with the group norm. While none of these results is startling, they add something to our knowledge about influence.

Gerard (42) reports a study on direction of communication and pressures toward uniformity in groups which consist of majority and minority subgroups. His findings replicate the earlier findings by Festinger & Thibaut (35) on direction of communication, that is, within each subgroup the majority of the communications were addressed to those whose opinions were most divergent from their own. The most interesting finding from this study, however, concerns the comparison of the majority and minority subgroups. There is good evidence to show that pressures toward uniformity are stronger for the members of the minority subgroup.

March (84) reports an investigation of influence between husband and wife using the procedure developed by Strodtbeck (130). He finds that husbands tend to influence their wives most on issues connected with labor, next most on issues connected with foreign affairs, and least on issues connected with local affairs. This order may be a complete artifact of the selection of husband-wife pairs for study. They were selected on the basis of the wife being a member of the League of Women Voters. If this selection does determine the order of influence we have learned very little from the study.

The material on reference groups in relation to attitude change is very sparse this year. Sherif (119) stresses the importance of the concept of reference groups for understanding stability and anchoring of attitudes and urges the importance of studying conflicting reference groups. This may indeed be good advice. Stern & Keller (128) report an analysis of interviews with 200 French people in terms of spontaneous group references in the interview. The analysis scheme follows the notions of Merton & Kit (93). They find that mentions of various reference groups by members of different social classes are quite different from what might be expected in the United States. They explain this in terms of relative lack of upward mobility in France. Rommetveit (110) reports an extremely interesting study on religious attitudes and ideas about sex roles among school children in Norway. He is primarily concerned with studying the "enduring social pressures" which exist, and the reference groups which are effective in changing and maintaining attitudes. He finds large differences between recognized official attitudes and held attitudes. He also finds that the held attitudes are closer to the perceived attitudes of agemates than they are to what they perceive parents or ministers as expecting of them. These data can be dealt with effectively in terms of reference groups.

One particularly interesting and encouraging aspect of the notion of reference groups is that it does seem to lend itself to cross-cultural comparison and research.

SOCIAL PERCEPTION

There has, for some time, been considerable interest in the problem of the perception of feelings in others or of the opinions of others. It is sometimes

referred to as social perception, sometimes as empathy. Characteristically this research measures how a number of persons feel toward some issue or towards each other, and how each of them thinks the others feel. Various indices can then be calculated and examined, the most frequent ones being measures of accuracy of the estimations of how others feel. Such indices are somewhat complicated, however, by the fact that variations in how similar the persons actually are in their feelings and variations in their tendency to assume that others feel the same way they themselves feel, will affect accuracy. Thus Tagiuri, Blake & Bruner (135) find that there is greater than chance amount of guessing that others feel toward you the same way you feel toward them. They also find that perception of who likes them is accurate above chance, but this is not the case with perception of who dislikes them or of who feels neutral toward them. This greater accuracy of the perception of "liking," the authors feel, is perhaps directly attributable to the first mentioned finding. In another study, Bender & Hastorf (9) find that accuracy is related to actual similarity among persons. If a measure of accuracy is corrected for actual similarity and for assumed similarity, that is, the tendency to think others feel the same way you feel, there turns out to be no relation among the measures.

Somehow these studies do not seem to help us much in our understanding of the processes involved in social perception. The same thing may be said of a study by Kirkpatrick & Hobart (69), who show that couples who know each other well can estimate each other's attitudes better than chance, and of a study by Soskin (124) who makes the point that information from restricted situations can lead to great bias in social perception. What we seem to need in this area are studies which will cast some light on what determines various social perceptions and what goes on during the process of acquiring such percepts. There are two studies which make interesting inroads in such directions. Jordan (63) studied the pleasantness or unpleasantness of different possible perceived relations among three persons. He finds, for example, that a situation where two people you like are perceived as disliking each other is quite unpleasant. Further research on how such unpleasant or uncomfortable situations affect further social perceptions could be very valuable. Along different lines, Hastorf & Cantril (51) report a study of students' perceptions during the viewing of a movie of a football game. The game itself had presumably been a rather rough affair and, following the game, both schools accused the other of purposely being too rough. The movie was shown to a sample of students at each of the two colleges, Dartmouth and Princeton. Princeton students saw twice as many infractions of the rules by Dartmouth in the movie as did Dartmouth students. In other words, school loyalties or preconceived opinions or both strongly affected their perceptions while viewing the movie.

Another series of studies attempts to deal, not with the determinants of social perception, but with the effects of it on various aspects of group functioning. Gage & Exline (41) examine the relationships between accuracy of

social perception and sociometric measures of effectiveness in a discussion group. No consistent relationships are found, however. Bell & Hall (8) find small positive correlations of .25 and .18 between a sociometric measure of leadership and two different measures of accuracy of perception of others. Norman (100) finds a U-shaped relationship between accuracy of perception and sociometric status with those both high and low in sociometric status being somewhat less accurate in their perceptions of others. These different studies do not seem to add up and, in fact, seem to contradict one another. Gage (40) comments extensively on the fact that studies such as these have shown very inconsistent results. Pointing to the study by Chowdry & Newcomb (22) as showing that there is a relation between leadership status in a group and accuracy of social perception for items that are relevant to the group but not for items which are irrelevant, Gage suggests that the variable of relevance is perhaps the explanation for the inconsistent results of various studies. Perhaps this is true. Indeed, such a variable looms more and more importantly in social psychology, especially with respect to group processes. It has been shown experimentally to be important in connection with influence processes, for example, by Schachter (114). Relevance, or some analogous concept, is mentioned more and more in writings by social psychologists as an important factor and certainly plays a strong implicit role in designing studies. The major difficulty, however, lies in the vagueness of the concept and the frequent inability to say beforehand what is and is not relevant. The specification of the concept and accumulation of data concerning it is necessary.

GROUP STRUCTURE AND GENERAL FUNCTIONING

Two of the vaguest concepts in social psychology are "group" and "group structure." These words get used to denote a great variety of different things with relatively little specification of the exact meaning. They undoubtedly, however, refer to important variables and important phenomena, and it is consequently refreshing to have a series of studies appear which really begin to introduce specificity and conceptual clarity into the general problem area denoted by these terms.

Horwitz (55) reports an excellent study pinning down the matter of individual acceptance or nonacceptance of group goals. He creates a situation in which a group goal, as such, exists because the group must decide whether or not to complete an activity. He then makes very ingenious use of the Zeigarnik effect to reflect evidences of correspondence or noncorrespondence between the group goal and individual goals. The Zeigarnik effect is generally accepted as showing that if a person has a goal, and the relevant task (completion of which would mean reaching the goal) is interrupted, there will be a greater tendency to recall that task. Consequently, in this group situation, where there is an explicit overt statement of group goals, the relative recall of completed and interrupted tasks by individuals should reflect their acceptance or nonacceptance of the group goal. The results show clearly and unequivocally that this is indeed the case. Where

the group goal is identical with the individual goal, or is accepted by the individual, the Zeigarnik effect is greater than where the group goal is rejected by the member. Another study by Horwitz & Lee (56) helps pin down this interpretation. It is to be hoped that more studies like this will eventually remove the vagueness and ambiguity from notions like "group goal," "identification with group goals," and the like.

Harvey (50) reports an ingenious study which helps clarify the notion of status relations in a group. A number of informal groups were first investigated in order to determine, within each group, one member who held clearly high status, one who held clearly low status, and another who held intermediate status in the group. These three persons were then tested together in a level of aspiration situation where they had to estimate their own future performance on a task and were also asked to make similar estimates for the future performance of the other two. The discrepancy score, that is, the size of the difference between actual performance and estimated future performance, is the major measure employed. The author finds that the higher the status, the greater is the discrepancy score when estimating own performance. The estimates others make of a given person's future performance are also in line with status. The others estimate very high future scores for the high status person and very low ones for the low status person. There is, consequently, in the level of aspiration behavior, a clear reflection of one's own knowledge of one's status in a group and of the acceptance of this status by the others in the group. This type of measurement should prove very profitable for investigating status relations.

Mills (95) and Strodtbeck (129) report studies attempting to measure the effects of power relations among members of three-person discussion groups. Mills finds that in three-person peer groups two of them tend to pair off against the other or to exclude the other. He also finds that those patterns of interaction where the two highest participants support each other are stable in that the pattern does not change over time. Since, however, the measure of power in the group is simply the amount of participation, the findings are circular insofar as they are interpreted in terms of power relations. Strodtbeck attempted to replicate these findings in a situation where there was an independent measure of power relations, namely, three-person groups consisting of father, mother, and adolescent child. He, however, finds totally different results from those which Mills reports.

Other studies relating to group structure are: Swanson (134) shows that groups of three persons who have had practice on a task follow new suggestions less readily, communicate less with one another during performance, and show greater differentiation of who does what than groups which have had no previous practice. James (60) observed group size among pedestrians, in shopping centers, playgrounds and beaches, and the like and finds that the group size varies usually from one to six. Willerman (139) shows that girls who are active in sorority affairs are higher on academic achievement, self-

confidence, and number of friends than those who are not active. Bonney (12) shows that boys choose girls sociometrically just about as much as girls choose boys. Horrocks & Wear (54) show that sociometric choices of college students are somewhat different when they are choosing a chairman than when they are choosing a social companion. Comrey (27) shows that two persons working interdependently on a manual dexterity task work at a speed intermediate between the individual performance speeds.

The relationship between group structure and productivity or effectiveness of a group has for a long time been a major focus of research. Much of this research, which deals with leadership in the group as related to productivity, we will review in the next section. There are a number of studies which look at other aspects of group organization which we will examine here. Rice (106) reports the results of an experiment in a factory in India where textiles are woven. The work had previously been organized so that, while many workers were interdependent in producing the finished product, no stable work groups existed. The organization of the factory was then changed so that there were stable, smaller size, work groups. This change was received with enthusiasm by the workers and resulted in markedly increased productivity in the factory. Rice proposes the interesting hypothesis that interdependent jobs require stable group relations among the workers performing those jobs. If this hypothesis turns out to be correct it starts providing an answer to the perplexing question of when such group organization is or is not effective.

Macy, Christie & Luce (81) report a study following up the results of the work of Bavelas (7) and Leavitt (75). These studies have all examined the effect on problem solving of certain different arrangements of permitted communication linkages in a group. The problem used is typically one where, once all information has been shared or centralized, the answer is very easy to come by. The major problem for the group is to find a way to get all the information accurately together. The most recent study shows that those patterns of communication linkage which most easily permit the checking of incorrect transmission of information are most effective in reducing errors. This is interesting in that what may be effective for speed or quantity may not be effective for accuracy.

Lastly, in this section, we would like to mention a very stimulating theoretical discussion by Cohen (26) in which he raises the question as to when, if ever, one may expect groups to be creative or original in their functioning. This is an important problem, an answer to which would help explain many matters of where groups can or cannot be expected to function well. It is interesting that, in spite of all that has been said and written about such things, Cohen reports that the existing literature is not very helpful in suggesting answers to his question. He discusses the interesting hypothesis that pressures toward agreement in groups may serve as an effective deterrent to creativity and originality.

LEADERSHIP IN GROUPS

Leadership has, of course, been a favorite problem for a long time both because of its practical usefulness and because of its theoretical importance. The problems which have been attacked range all the way from simple questions about the situation in which the leader finds himself through problems concerning how the leader relates to the group and his effect on the group to problems on training of leaders. Before reviewing the research which has been reported on this variety of problems during the last year, some general comments are in order. There seems, to me, to be a huge difference between the situation where someone in a group or organization is given power or responsibility to supervise or lead, the fact that he has this special position being recognized by the others in the group, and the situation where there is no such role differentiation but persons who participate more are regarded by the researchers as though they are leaders. There is sometimes an implication in the literature that the above distinction is a difference between formal and informal leaders but that does not seem correct. The study mentioned in the previous section by Harvey (50), for example, is a study involving informal leaders or informal status. This means that there is no formal designation of status or that the persons who are really accepted by the group as in the status or leadership position are different from those formally designated as such. Informal leadership position, however, must involve recognition of this and acceptance of it by the others in the group. Formal leadership designation, while it, of course, must involve the perception on the part of others that someone has been thus formally designated, need not involve acceptance of the person as a leader. This distinction is made admirably clear in a study by Gross, Martin & Darley (45) who studied the leadership structure in a number of student co-operative living houses. They separate the houses into two groups, those in which the formally designated leaders are indeed supported and accepted as leaders by the girls and those houses in which this is not the case. The former are instances in which the formal and informal leadership is identical. With a small number of cases (13 houses altogether) they find some tendency for those houses where the formal leaders are also the informal leaders to be more productive in a contest.

Amount of, or type of, participation in a group, however, is not leadership in the same sense as above, and we include it in this section only because those reporting such studies call it leadership. Shears (118) had school classes conduct their own English lessons and also plan a party. He finds that in 13-year-old groups those who contribute many supportive statements tend to be the same in both situations while among 16-year olds this is not true. This result may simply be attributable to the fact that among the younger people verbal ability has more of a general impact. The fact that verbal ability is an important determinant on this type of measure is also shown in a study by Bass *et al.* (5) where the only measures (out of 41 variables) that are found to correlate appreciably with participation of a steering nature in

a discussion are "verbal ability" and "ascendancy and sociability." Klubeck & Bass (70) report that girls who are initially high on participation which attempts to steer others in the group discussion become slightly higher after a half hour talk which explains to them that such behavior is good, while girls initially low do not "benefit" from this half hour talk. This is again understandable if we imagine that those with high verbal ability can use it in whatever direction they desire while those with low verbal ability can do relatively little in the situation.

Somewhat related to the above studies are those reported by Carter (18) and by Cattell, Saunders & Stice (20). These studies attempt to explore leadership, productivity, and other variables by means of putting groups in a variety of relatively free situations and measuring many things about them. The latter article, for example, measures 93 variables and, after a factor analysis, comes out with 15 factors. Since these studies must still be considered exploratory in nature, we shall have to wait to see, in the years ahead, whether or not they prove fruitful.

Let us now turn to a consideration of the work on formal leadership. Seeman (117) reports a study relating to role conflict in leaders. He reasoned that since there are frequent instances where cultural ideologies conflict for a leader with expectations expressed by his subordinates as to what he should do, role conflict should be present for the leader. He presents some evidence obtained from interviewing school supervisors that they do perceive or experience such role conflict. As we have mentioned earlier in this chapter, the concept of role conflict may be a fruitful one for social psychology, but we will need empirical studies of the effects of role conflict before this can be determined. The study by Seeman presents no such data.

Related to the problem of role conflict, although the authors do not talk about it in these terms, are the studies by Halpin (47) and by Fleishman (36). The former study reports the results of questionnaires about the aircraft commander of bomber crews given to the crews and to superiors of the aircraft commanders. They find that the satisfaction of the crew with the aircraft commander correlates positively with how considerate they think he is. The ratings which superiors give to the aircraft commander, however, correlate negatively with how considerate the crew thinks he is. In other words, crew members and superiors put exactly opposite evaluations upon the aircraft commander being considerate. Again, however, there are no data on the importance of this factor. The second study mentioned above shows that in an industrial setting workers expect more "consideration" and less "initiating structure" from an ideal foreman than foremen say they actually give. Again, then, there is the basis for role conflict, but again, there are no data concerning what it makes a difference for and how much of a difference.

The research on the relation between leaders' behavior and the productivity and effectiveness of a group continues to be in a state of contradictory evidence. Lawshe & Nagle (74) report a correlation of $+ .86$ between man-

agements' ratings of the productivity of 14 departments and workers' questionnaire responses of how their immediate supervisor behaved toward them; the more favorable the workers' reports about the supervisors' behavior, the more productive was the department. The authors regard this as confirming the results reported several years ago by Katz, Maccoby & Morse (65) who found a tendency for supervisors who exercised close supervision to have less productive departments than those who exercised general supervision. Morse (97), however, reports a new study of white collar workers in which it is found that general supervision as opposed to close supervision increases morale but decreases liking of the job; satisfaction in the job is related to aspirations and attainment of these aspirations; but none of these shows any clear relations to productivity. Torrance (137) reports an experiment in which the experimenter, acting as leader, compares the effect on subsequent performance of several different ways of conducting a critique of previous performance. He reports that the group performance on problem solving improves where the experimenter acts as an expert and evaluates the group's previous performance. But the improvement is just as marked where the leader acts as a nondirective discussion leader provided the group is given a number of rating scales as a guide for their discussion. Where these rating scales are omitted or where the leader leaves the room and allows the group to discuss the performance among themselves, the results are indistinguishable from those of the control groups. In this study, then, whether the experimenter is nondirective or highly directive does not seem to be a relevant variable at all.

Finally, on this problem of relation of leader behavior to productivity, McCurdy & Eber (89) report a study contradicting earlier work by Lippitt (77). Lippitt found that when the leader was democratic, groups were more "productive" than when the leader was autocratic. In a previous experiment, McCurdy & Lambert (90) found no difference at all in problem solving effectiveness between groups that were treated democratically or autocratically. The present study was designed to test the hypothesis that democratic group treatment would be more effective for democratic people while autocratic treatment might be more effective for authoritarian people. Authoritarian and nonauthoritarian persons were then selected on the basis of the California F scale. Four conditions were set up in which autocratic and democratic treatments were each used with authoritarian and nonauthoritarian subjects. Again no differences among conditions emerged except for a slight tendency for the condition where democratic treatment was used with authoritarian subjects to be worse than any of the other conditions. It would seem to this reviewer that it is high time this whole area was thought through and some attempt was made to formulate hypotheses concerning the determinants of when these factors of leadership behavior should and should not be related to performance of the group.

One study which may cast some light on this problem is reported by Maier (83). Each of his groups consisted of four persons, one of whom was

arbitrarily assigned as leader. Half of the leaders received training in how to carry out a permissive discussion while the others received no such training. The problem the groups had to reach a solution on was one where the facts were quite clear but the obvious solution was rather unpalatable to the members. In the control groups there is flat rejection of the obvious solution in 50 per cent of the cases while this happens in only 5 per cent of the experimental groups. But the difference does not show itself in acceptance of the obvious solution; here both control and experimental groups are equal. The difference is, that in the experimental condition, where the leader has received the training, a large number of groups reach new compromise solutions. This does not happen in the control groups. This type of emergence of some new idea would not have shown itself at all if the situation were such that "productivity" were measured by per cent of acceptance of the clear and obvious solution.

SPONTANEOUS COMMUNICATION

There is perhaps no justification for having a separate topic heading for communication. Communication and verbal behavior is, of course, one of the major ways in which people are able to function together, but, when communication occurs, it is always about something and is hence relevant to some other content area. There are, however, a number of studies which discuss or point out pertinent things about the process itself with less emphasis on the content. It is convenient to group these studies together.

Burns (16) reports an interesting study of communication over a long period among the four top people in one department of a factory. The data were collected by having the four people keep records of every instance of communication in which they engaged. Some of the more interesting findings which emerge are: There tends to be a disproportionate amount of communication among people within the same level in the hierarchy. This may indicate some restraints against communication between levels. When communication does occur between levels it tends to be initiated by the higher level. There is also much disagreement concerning what they talk about. The person from the higher level will frequently report that he gave instructions and orders to the person at the lower level while the latter, concerning the same episode, reports receiving information and suggestions. These results are tentative, but they provide an interesting set of suggestions for investigating the effect of hierarchical structures on communication. In another article Burns (15) offers the additional hypothesis that banter and joking hostility during communication represents an effort to maintain, at the same time, two conflicting social roles.

Bauer & Gleicher (6) stress the importance of word-of-mouth communication as a source of information. They analyzed a large number of interviews and questionnaires from Soviet refugees with respect to their sources of information and news while living behind the iron curtain. Their data indicate a very high dependence on rumor for obtaining information and an

astonishingly high trust in rumor and word-of-mouth communication. The suggestion, of course, is that the importance of word-of-mouth communication exists because of the government control over the mass media.

INTERGROUP RELATIONS

The problem of intergroup relations, while an extremely important one, has in a strange way been very neglected in social psychology, and this neglect continues. There are a great many problems connected with the relations between groups and the behavior of groups with respect to one another. Most of the work on intergroup relations, however, continues to be confined to minority group problems. It is also probably fair to say that no new insights into these problems have emerged recently.

A few mainly methodological studies have been reported. Sommer (123) administered a dialogue completion test of attitudes toward Negroes originally devised by Brown (13). He reports good correspondence between this test and an ordinary questionnaire. Prothro & Miles (103) administered a revised Bogardus scale of social distance in the South and report that the scale ranks correlate $+ .84$ with the original ranks obtained by Bogardus. Langner (73) reports administration of a test of prejudice which involves checking traits which apply to specific nationality groups. It was given to Indians, Spanish, and Anglos in a Colorado community. The results show the Anglos enjoy highest status and the Indians, lowest. This reviewer hopes that with the large number of measuring instruments that are in existence the field can now proceed to substantive research.

Two studies have appeared this past year which, in essence, examine the effect of a *fait accompli* on intergroup attitudes. Clark & Swanson (25) report the results of interviewing residents of a neighborhood concerning their attitudes toward a low rent housing project which had recently been built adjoining this neighborhood. They compare their results with identical interviews conducted in the same neighborhood before the project was built (24). They report that there was a significant shift toward more favorable opinion of the project. Rose, Atelsek & McDonald (111) interviewed residents of neighborhoods containing one Negro family. They report that those living close to the Negro family had more contacts with them and also showed a tendency to be more favorable to this Negro family than those living farther away. The same point which these two studies seem to make, namely, that a *fait accompli* can have favorable consequences for intergroup relations also emerges strongly in a review of instances of desegregation by Clark (23). He finds that there are favorable consequences to most instances of arbitrary elimination of segregation. This is, however, by no means always, or nearly always, the case and none of the studies cast light on the determinants of when it does or does not have specific consequences.

Sherif & Sherif (120) report a highly preliminary and tentative but refreshing attack on the problem of intergroup relations. In addition to reviewing the literature on intergroup relations and examining various theoretical explanations of prejudice, they report an attempt experimentally to create

intergroup conflict so that the determinants of such relations may be examined. The experiment, conducted in a boys' summer camp, is intriguing. In its present state it is little more than a case study but it holds promise of further fruitfulness.

CONCLUSIONS

It should be clear to anyone who has read this review, and the preceding five or six years of reviews, of the area of social psychology and group processes, that it is hardly one area but rather many areas. The result of this is to make it difficult to review it all coherently. Many of the problem areas have been, and continue to be, developing rapidly and, under such circumstances, it is also difficult correctly to assess the importance of a given piece of research or even of a whole area of research. I have made selections on both counts, that is, I have omitted many individual pieces of research and some whole areas which someone else might have included. I can only hope my errors of judgment have not been too great.

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INDUSTRIAL PSYCHOLOGY^{1,2}

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INTRODUCTION

The publication of Lanier's comments (59) on earlier volumes of the Annual Review has produced feelings of self-consciousness and frustration in the writers. It must be admitted that his discontent with the lack of integrative evaluation and systematization found in the previous industrial psychology chapters is not likely to be removed by this one. As he points out, the literature in industrial psychology is both heterogeneous and "atomistic." If there is much thinking going on about the broad theoretical basis of psychology's efforts to improve industry's utilization of human resources, it does not show itself in the literature of 1953 to 1954.

Neither in textbooks nor in statements of research problems do we find explicit and organized efforts to seek out or to state the general principles which generated a particular attack on an industrial problem. Unfortunately when this rarity does occur it is only too likely to be accompanied by a disregard for an evaluative step. Thus, "principles of learning" have been systematically employed in developing training courses for which the claims are great and the evaluative data meager. Those who seek rationale in the development of selection predictors seem less likely than their empirically-minded associates to bother with validation. And we are all too familiar with the wide and uncritical acceptance of "what psychology tells us about human relations." Of course, those who have subjected their hypotheses and their consequent procedures to empirical evaluation have had a rather discouraging time of it.

All of this may mean that the principles are too scarce or too faulty. Or, it may mean that the methodology available to the industrial psychologist in applying and evaluating the principles is too coarse and inaccurate to reveal their potential. If this is true, and there is evidence that it is (e.g., see the section below on the criterion problem), a good case can be made for the industrial psychologist who concerns himself mainly with methodological development and refinement. In this review, the authors have attempted to place the emphasis on work of this nature. This implies a belief that when we have advanced methodology, if only to the point where we can make accurate comparisons of the relative effectiveness of alternative techniques, we will be ready to contribute to theory.

Unfortunately, as Lanier also points out, reference will be made to studies with "inadequate samples, uncontrolled conditions, and uncertain criteria."

¹ The survey of the literature pertaining to this chapter was completed in May, 1954.

² The following abbreviation has been used in this review: MMPI (Minnesota Multiphasic Personality Inventory).

Until industry allows itself to become a laboratory these conditions are likely to exist. This depends to some degree upon the incidence of "executive daring" in top management. It also depends on the willingness of industrial psychologists to seek and argue for experimental controls where possible. Psychologists with the Armed Forces have shown that this can be done and that the results are of value not only to their sponsors but to the field in general. Unfortunately, too little of their work is seen by other investigators. It is not, for example, as well represented in this and previous reviews as it should be. This is also true, if in a lesser sense, of many investigations in private industry. Much good work of a long term nature, particularly in larger companies, receives most of its circulation orally, if at all. The need for progress reports and descriptions of programs with emphasis upon both practice and research is pressing. A praise-worthy attempt to alleviate this situation has been made by two of the major journals in the field. *Personnel Psychology* has initiated its "Validation Information Exchange" and the *Journal of Applied Psychology* its "Applied Psychology in Action."

A significant development which deserves the attention of industrial psychologists is the emergence of "operations research." The papers of Goodeve (39) and Herrmann & Magee (47) provide general descriptions of the problems and techniques characteristic of this field. Magee (67) and Levinson (62) give illustrations of attacks upon specific problems.

Industrial psychologists must or should be singularly sensitive to their standing with the public and to their impact upon the public's thinking. Indeed, to some degree, their progress must be judged in this light. There appear to be no reasons for complacency in this connection. The business man's prestige magazine, *Fortune*, last year published a serious and sympathetic account of a physiognomy selection system with little reference to the fact that psychologists are better equipped to evaluate it than prominent industrialists [Stryker (97)]. This is not only *Fortune's* or the business man's fault. The man in management who reads Seashore's accurate description of psychologists' ethical code (86) will find that psychologists must be trained, must operate within the boundaries of their competence, must apply their methods and knowledge with proper scientific modesty, etc. He does not find out that psychologists should know how to institute procedures for demonstrating whether their or other experts' advice is effective, and should set up such procedures or explain why they do not. He needs to be told this. Perhaps industrial psychologists need to be reminded of it. For this reason, priority in this review has been given to reports of experimental and evaluative nature.

CRITERIA

The criterion problem continues to lead all other topics in lip service and to trail most in terms of work reported. It seems probable that almost all investigators now recognize the importance of developing acceptable criteria and submitting them to the greatest scrutiny and correction. Unfortunately, a reviewer must also conclude that the pressure of getting things done is still wooing many into the convenient device of accepting the criterion at hand

and hoping it will turn out all right. In all justice, however, it should be pointed out that many industrial psychologists show in informal discussion that they worry more about and do more work on criteria than the literature would indicate.

Nagle (78) illustrates this continued paucity of publication when he presents a review of the last decade's literature on criteria based on 31 titles. His discussion centers around three problems: relevancy, reliability, and the combination of multiple criteria. Readers may feel that the treatment accorded the first two of these problems is somewhat elementary. While it may be true that in criterion contamination, "the real problem lies in anticipating the occurrence of . . . extraneous variables," it seems the height of optimism to conclude that "When one is aware that a certain extraneous element is likely to occur in a criterion, the influence of the extraneous element can usually be controlled experimentally or statistically." The treatment of reliability leaves even more to be desired. Nagle's discussion of the sub-criteria problem and methods of their combination is of greater value. However, some readers would quarrel with his agreement with Toops (106) that a unitary criterion score is indispensable. Situations in which sub-criteria are unrelated or even negatively correlated are not unknown. To put these together in a single score would appear to make the prediction problem more difficult if not insoluble as well as to hide some important and heuristic relationships that do exist. It is unfortunate that the method of multiple cut-offs is mentioned but not discussed. The "dollar" criterion [Brodgen & Taylor (13)] is described as solving most of the difficulties of criterion combination, with no indication that the method's impracticality serves as a source of frustration to many industrial psychologists.

While not supporting the use of a global criterion, the work of Rush (83) gives pause to those who would attempt the combination of sub-criteria. He shows that a set of objective and subjective criteria of success in a specific selling job cluster into four relatively uncorrelated factors. Composite criterion scores for each of these factors were derived and differential validities based on 10 predictors obtained. The problem of how such factors might now be combined into a unitary criterion may or may not be important. Perhaps their rather informal combination into "over-all" criteria accounts for much of our lack of success in this area. Baxter, Taaffe & Hughes (4) present a study in which the importance of examining sub-criteria is recognized. They employ an objective test of knowledge, a job satisfaction questionnaire, supervisory ratings, costs, total new business sales, and job termination in the evaluation of three different training procedures for insurance agent recruits. A study of this type may put the problem of criterion assessment in bold relief if, for example, one training method produces superior performance on one criterion while another method is superior on the basis of a second. Unfortunately, none of the methods produced different results for any of the criteria so we are deprived of the opportunity to see how the authors would have attacked the problem.

Vallance, Glickman & Suci (109) and Hitch (48) contribute some good

thinking on the problems underlying the selection of criteria. The former emphasize the problems encountered in considering intermediate criteria within an executive hierarchy. The latter discusses criteria from an operations research viewpoint, pointing out the dangers in assuming that sub-criteria are consistent with ultimate criteria, or failing to make allowance for the effect of changes in sub-criteria upon some other aspects of the ultimate criterion.

Among objective criteria, Tiffin & Phelan (104) note that turnover is receiving wider attention and ascribe this shift in emphasis to the increase in labor mobility and the tendency for greater standardization of output with concurrent loss in the variance of productivity measures. Certainly, many investigators are recognizing that the prediction of productivity may involve a quite different rationale than the prediction of job survival. The nature of the two problems may shift continuously and differently with economic conditions and may therefore require different predictors at different times. This is one of the least comforting concepts for research workers in this area. The difficulty is nicely illustrated by Behrend (5) who shows that there is a direct relation between the level of employment and the level of labor turnover and that this relation substantially transcends differences between regions or between individual companies or institutions. Thus, the "kind" of turnover one is using as a criterion may be quite different from time to time. Absenteeism is also shown to be sensitive to the over-all employment picture. The complexity of turnover is also stressed by Thomas (103) whose interviews with a large sample of British workers lead him to conclude that "We have got to do a great deal more work in this subject before we can begin to appreciate the complex of motives and circumstances that go to make up a man's mind when he proposes to change his job."

Metzner & Mann (69) studied an absenteeism criterion in relation to employee attitudes. They argue on logical grounds that frequency of absenteeism is superior as a criterion to actual days lost. They also find the former to relate more highly to stated worker attitudes.

No review of this year's work in industrial psychology would be complete without a mention of Mueser's study of a related criterion, i.e., punctuality (75). The discovery that rain does not make workers late but that fine bright mornings do may show us that still another source of criterion contamination must be contended with, but the concept that workers pause on a fine spring morning to pick a crocus has its own merits.

An objective production measure (curve of output) as a criterion of boredom or monotony is examined by Smith (91) who found no characteristic work curve for individuals or groups and no relation between subjective reports of boredom and work curves. She places a healthy emphasis on the way in which the quantity and temporal distribution of a worker's performance may be affected by his concept of how much should be produced. It is probably the recognition of the importance of subjective factors of this kind in

determining objective performance that has turned so many investigators toward subjective criteria. Indeed, while objective criteria are frequently used, there is little report of their systematic study. Accidents as objective criteria have received some attention [Leshan & Brame (61); Mintz (71)], but the great majority of methodological studies are in the subjective area.

Thus, Daniels & Edgerton (25), discouraged by the low reliability and high contamination in accident records, turn to ratings made by higher echelon personnel and by immediate supervisors to select groups of motor vehicle units which are widely separated with respect to safety of operation. The subjective classification of the units was then "validated" by comparing them on the basis of the percentage of vehicles reported by unit personnel to have been damaged during the preceding month. It is the authors' opinion that "criteria derived from ratings or rankings should be verified by showing them to be related to some critical behavioral aspects of effectiveness, acceptable to the psychologist, to the raters, and to the groups being studied." The argument will probably long continue between those who support this view and others who believe that if objective measures reliable and relevant enough to use for validating subjective measures are available, one would do better to use the former and neglect the rankings and ratings. It should be noted that the article of Daniels & Edgerton is one of the few representatives of attacks on the problem of group criteria. When it is considered how important this problem is in its own right and how closely it ties in with the area of supervisory criteria, it seems a pity that more activity of a methodological nature is not being reported.

Weitz & Nuckols (115) report an attempt to use criteria of group performance in evaluating supervisory effectiveness. Percentage of turnover and various measures of sales production per man within supervisory units were examined. The odd-even reliability of the turnover percentage during a four year period is reported as .77. Even presuming that this may be spuriously high because of external contaminating factors, it is encouraging. Judging supervisors by the objective performance of their men makes good sense. It may well be too soon to turn to subjective criteria even if objective group criteria have thus far been disappointing.

The Group Effectiveness Research Laboratory at the University of Illinois (40), under the leadership of Cronbach and Fiedler, has been approaching group effectiveness as it is dependent on the ways in which group or team members perceive and interact with each other. Emphasis has been placed on the effects of the members' tendencies to perceive personality similarities or differences among their co-workers on team performance. Formal and informal leaders and "keymen" have received particular attention. These investigators have apparently recognized that a complex problem may demand a complex and imaginative approach. Their future findings should be a matter of great interest to all who are involved in the field of leadership and group performance.

Moore & Smith (72) contribute an article of significance to those who must grapple with criteria for supervisory personnel. Their comparison of the characteristics regarded as desirable for noncommissioned officers by officers and by enlisted men leads them to warn those in the industrial field that supervisory criteria acceptable to management may be regarded as irrelevant or unacceptable by workers or union officials. Here we have the sub-criteria combining problem with a different and difficult twist.

Springer's (94) results on the comparison of ratings by supervisors and co-workers on the promotion fitness of candidates can be interpreted to support the notion that both jobs and men may be perceived differently by labor and management. She finds very little agreement between the two groups and there is only slightly better agreement among the co-workers. The agreement among supervisors is considerably better, but one wonders if this may not reflect a greater opportunity for the influence of a spurious factor. In short, the findings could be interpreted to mean simply that the ratings by both groups are too unreliable to tell us anything. Ratings were made on a number of aspects of performance as well as over-all fitness. As usual, the sum of the individual ratings correlated highly with the over-all item. Halo remains around our heads.

Work aimed at identifying the characteristics of raters and situation which are related to the validity of ratings shows promise. Schneider & Bayroff (85) find that army officers who make more valid ratings of others (i.e., agree best with the mean of 20 raters) have certain common characteristics. They are rated highly by their peers, have higher academic standing at Command and General Staff School, have higher scores on the Officer Classification Test, and exceed the academic standing predicted for them on the basis of the correlation between the test and academic grade. Some interesting comparisons of signed and unsigned ratings and forced-choice techniques are also given.

Whitla & Tirrell (118) used the score on a Flight Mechanics Job Knowledge Test as a criterion for rater validation. They found that immediate noncommissioned officer supervisors gave ratings in substantially higher agreement with the test scores when asked "How much does he know about his job?" and "How well does he do his job?" than when asked "How well does he get along with others?" This was not true of groups more remotely associated with the workers. This appears to support the earlier work of Ferguson (30) who showed that the accuracy of ratings is related to the degree of acquaintance. It is refreshing to find some situations in which judgement based on more information is actually better than judgement based on less!

French (36) continues his excellent work on the "adaptability for service" criterion. Based on ratings of United States Coast Guard Academy cadets by officers, upper classmen, and classmates, a measure has been derived and demonstrated to be single, universally accepted, and fairly reli-

able. Unfortunately, thus far it has proved to be substantially unpredictable, although slightly related to course grades, athletic proficiency, and memory. The author believes that the criterion, while realistic, is also very complex. He indicates that future work will go in the direction of breaking it down into simpler parts and of examining predictors based upon past (therefore complex) performances. This is one of the few studies where logic has prevailed and the criterion problem has been given its rightful priority.

The literature on the forced-choice approach to the rating problem continues to grow. A significant methodological contribution is made by Berkshire & Highland (10). These authors compare six kinds of forced-choice forms for rating instructors under experimental conditions and four types of forced-choice and a graphic rating scale under operational conditions. The comparisons are made in terms of resistance to leniency, split-half reliability, validity, and relative acceptability to raters. Validity is defined as correlation of forced-choice scores with supervisors' rankings of over-all instructor ability. This secondary criterion is employed because of the absence of "a composite of scores resulting from measures of average student gain under different instructors." In justice to the authors, it should be added that they have been making considerable efforts to obtain such measures.

Berkshire & Highland's article may be best summarized by saying that anyone interested in ratings, and particularly in the forced-choice approach, should read it. It will be of primary interest to some to find that the old graphic rating scale "exhibited relatively little bias, and had as high validity and reliability as the best of the forced-choice scales." Others will be interested in the comparison of forced-choice techniques under experimental and operational conditions while, still others, will wish to examine the comparison between a "favorableness index" and a "preference index" in the rationale of forced-choice construction.

Taylor, Schneider & Symons (101) give a good example of the importance of studying criteria in validating and understanding another criterion. Thirty-two forced-choice tetrads and 13 graphic rating scales were administered experimentally to optical goods salesmen. A criterion of bonus earnings (paid following the attainment of an assigned sales quota) was used, and an empirical key constructed for the forced-choice tetrads. Cross-validation gave a validity of .01. Re-examination of the criterion indicated to the authors that "the salesman's salary was based upon the company's carefully considered judgement of his worth and ability, and that quotas had been set on the basis of full consideration of the richness of the territory and the past performance of the individuals concerned." This led the authors to conclude that the bonus, since it was based on a quota corrected to some degree for the salesman's ability, was not a good criterion while the basic salary was. A revalidation using basic salary was determined and held up at .43 upon cross-validation. The graphic scales were factor analyzed and yielded only one factor. However, for various reasons five of the scales were added to

the forced-choice section and produced a shrunken multiple correlation with salary of .45. The regression weights indicated a scoring formula of twice the forced-choice score plus the graphic.

The authors are quite understandably exhilarated to find that the application of this weighted total to a group of salesmen of another of the company's products produces a validity with salary actually higher than that on the original cross-validation sample. They present the interesting hypothesis that generally valid evaluative devices might result from scale construction based on material obtained from representatives of a variety of industrial organizations. However, as the authors point out, the salaries used as criteria were determined on something less than an objective basis. This study may show only that what appears at first glance to be an objective criterion is really only a rating in disguised form and that (alas) ratings may be unreliable and irrelevant and yet agree substantially, if spuriously, with other ratings even when obtained under different conditions and at other times.

Tests and interviews are being widely used for describing individuals and making recommendations for their selection, training, supervision, promotion, and guidance. This has resulted in an informal sort of validation where the criterion is whether the employer or the appraisee agrees with the description (whether he understands the trait names employed or not). Although the inadequacies of this criterion are manifest, it is a powerful one in selling management, as the many testimonials demonstrate. Clark & Owens (18) make an attempt to, at least, formalize the procedure by having judges rate 47 employees (selected by management as having "rather distinctive and unusual personalities") on such adjectives as "vigorous," "dominant," "stable," etc. Despite the method of selection of subjects, neither scores made on tests such as the Thurstone Temperament Schedule and the Bernreuter Personality Inventory, nor the Worthington Personal History Blank (an unstructured application blank used as a projective technique) showed significant association with the criterion. The philosophy underlying the use of criteria such as this one needs to be examined by the industrial psychologist. Granted that management enjoys hearing what kind of a man a worker is, do the descriptions signify much unless we know how they are related to performance?

JOB SATISFACTION AND MORALE

Two major positions might be taken with respect to work in the area of job satisfaction or morale. It might be argued that job satisfaction is an end in itself, and one should try to determine the factors which are related to satisfaction or dissatisfaction with the job. The other position is that we should determine the job opinions and attitudes which are related to criteria outside of job satisfaction, such as production or turnover. These two positions are rarely differentiated, possibly because it is often assumed that job satisfaction is closely related to performance on the job.

An example of the type of study which attempts to determine the factors which are related to job satisfaction as a criterion is that of Handyside (42). He describes what he considers a utilitarian approach to raising job satisfaction. He uses a question on over-all job satisfaction and then relates items from a revision of the Hoppock test to the over-all estimate. After computing a contingency coefficient for each item, he multiplies this by the per cent of the sample giving this response and comes out with a priority index. (Handyside holds no brief for this as a good statistical procedure.) This procedure leads to an ordering of factors related to over-all job satisfaction. For example, the highest priority index item for women is "Is your boss a woman?". "Yes" is the unfavorable response. That is those who say "yes" to this item are more likely to be "Not Happy" about their job. If over-all job satisfaction is to be considered an end in itself, it would be expected that in order to remedy this situation women should have male bosses. We do not know, however, whether (a) this would improve over-all satisfaction or (b) this would lead to any improved performance or reduced turnover.

A study reported by Wonderlic (122) also attempts to measure job satisfaction without relating it to performance. He uses an anonymous questionnaire which makes individual validation impossible. However, Wonderlic feels that the workers in the study are "... apparently convinced that their participation in these surveys are constructive, and brings them better working conditions and a better relationship with managers and fellow employees." His evidence for this is the fact that the per cent of returns to very similar questionnaires has increased in yearly surveys since 1948. Again it is to be noted that no attempt is made in this study to relate job satisfaction items to performance. However, some attempt is made to take corrective measures to raise the level of job satisfaction in regions where it is presumably low. It would be of interest if a determination could be made of the effect of changes of this kind.

Baehr (2) attempted to investigate the effect of several variables in employee attitude scales during the developmental stage of the Science Research Associates Employee Inventory. The variables studied were: the arrangement of items (categorized versus randomized), number of scale intervals, and scoring procedure. She found high correlations between the various kinds of scales and scoring systems leading to the conclusion that the simplest (3 point, categorized, unweighted scale) would be as efficient as the other types of scales studied. It should be pointed out that this conclusion was based on the degree to which the various scales gave similar results. No attempt was made to determine which scale was more valid in the sense of being a better predictor of performance. Metzner & Mann (70) have some evidence that there is no greater relationship between questions grouped on one topic as opposed to the same questions scattered throughout the questionnaire. The findings are in agreement with those of Baehr (2) although Metzner & Mann have some reservations concerning their own conclusions.

In another methodological study, Weitz & Nuckols (116) attempted to

validate direct and indirect questions as a measure of job satisfaction on a group of life insurance agents. The indirect questions consisted of asking the agents to estimate the per cent of other agents who thought (for example) that their training was good. The comparable direct question was a three point rating scale asking about their own training. Eight such questions were asked covering various job areas. The correlations between the direct and indirect questions were relatively high ranging from .42 to .69 on over 500 cases. However this does not solve the problem of which type of item is a better predictor of a performance criterion such as termination. The indirect items were scored and the correlation with survival was .05 (an insignificant relationship). However the score on the direct items correlated with survival to the extent of .20, significant beyond the 1 per cent level.

Other workers in this area have attempted to relate job satisfaction and performance. Morse (73) feels that the relationship between job satisfaction and productivity is very complex. She says that many of the factors which enter into the formula for predicting productivity do not enter into the one for predicting satisfaction. From this she concludes that high productivity may or may not be associated with high satisfaction. Her main interest is to determine the dynamics of effective organizations. However, she does discuss the problem of relating workers' attitudes to productivity. Morse feels that productivity and job satisfaction have one factor in common, i.e., strength of needs. She states that if other factors are held constant, "the individual with the higher need level will be the more productive." His satisfaction level will vary according to the amount of environmental return which he receives. We interpret this to mean that those with higher need satisfaction will be the more highly job satisfied individuals.

In a very interesting theoretical and experimental paper, Schaffer (84) describes an attempt to measure need strength, need satisfaction, and over-all job satisfaction. His findings indicate that need satisfaction as he measured it was highly correlated with over-all job satisfaction. This is in agreement with Morse's position. However, introducing a "correction" for need strength did not improve his prediction. This study raises the question of whether need strength is common to both productivity and job satisfaction. Perhaps, as Schaffer says, he is not accurately measuring certain needs.

Another attempt to attack organizational effectiveness was carried out by Wilson, Beem & Comrey (121). They tried to relate attitude to high, medium, and low work groups. The criterion used was the meeting of production standards. The findings indicate that both "low" and "high" shop personnel had similar attitudes concerning their supervisors, and these were unlike the "medium" group. The authors make some plausible, after the fact, hypotheses concerning the curvilinear nature of the results, but these will have to be further tested. Metzner & Mann (69) tried to relate job satisfaction to the criterion of absences. The findings of this study show that job attitudes are related to absences among blue collar workers and white collar

groups working at low skill level jobs. These findings were not substantiated for women workers nor white collar workers at high level jobs.

Lawshe & Nagle (60) related attitudes and ratings of productivity of work groups. They found a correlation of .86 between attitude of the work group toward the supervisor and the rated productivity of the department. These authors conclude that supervisors' behavior as perceived by the employees is highly related to the output of the work group.

Gadel (37) finds a relationship between job satisfaction and survival. In studying part-time female employees she found that a group of older part-time workers had higher job satisfaction and survival than did younger full-time workers in the same type of job. This may merely be an indication that older part-timers would not take a job unless they thought it would be satisfying. Gadel suggests that older workers are more easily satisfied in these jobs. The fact that it is part-time permits greater freedom in such things as working hours, and this may make for both lower turnover and higher satisfaction.

Undoubtedly there are many factors which relate to performance other than job satisfaction, and we are always confronted by the causality problem. Is the worker happy with the job because he is producing (and making money) or is he producing because he is happy? The answer to this problem is not solvable in the usual survey type of study. In order to evaluate this, an experimental design must be used where a change, hypothesized to improve job satisfaction, is instituted and the effect on performance measured.

Tydlaska & Mengel (108) used the MMPI² as a scale for measuring work attitudes. From military personnel they chose a "poor" work group (those who had been AWOL, were disciplinary cases, etc.) and from civilians chose a "good" work group (two years or more of satisfactory service in a chemical plant). They find significant differences in the MMPI scores between the two groups and also differences in 37 of 50 items selected as possible predictors. The fact that one was a civilian and one a military group may account for some of the differences. However, the authors point out that this is a suggestive study and cross-validation should be undertaken before anyone gets too excited about the findings.

Van Zelst & Kerr (111) attempted to measure attitudes toward merit ratings. Unfortunately, their sampling procedure is suspect since out of 50 companies contacted only 14 co-operated, and in these the firms selected the sample. No firm was represented by more than 36 cases while others had 14 or 15. They conclude that "Merit rating systems when applied with proper observance of democratic and human rights procedures will meet with definite approval and favor on the part of the employee." This, in spite of the fact that about 50 per cent feel that zero weight should be given to merit ratings when considering promotions or layoffs. Fifty one per cent have the opinion that merit ratings give them a feeling of insecurity and 71 per cent feel that merit ratings are to the company's advantage while only 3 per cent

feel the ratings are to their own advantage. These findings do not seem to jibe with the conclusion that there is definite approval. The attitudes toward merit ratings did not relate to two items on over-all job satisfaction.

Smith & Kerr (89) report that pay was most frequently mentioned in an exit interview as the reason for leaving. The report is based on 48 cases out of an attempt to obtain data from 200 companies. How representative the results are is a question, but it is interesting to find job satisfaction studies which do not relegate pay to a minor role. Perhaps the economics of the times is a factor in determining the reasons for termination. Where labor mobility is great and times are good it may be that job satisfaction studies will show results quite different from those obtained under less favorable economic conditions. The problem of pay is discussed quite fully by Viteles (112). His book, *Motivation and Morale in Industry*, also includes excellent summaries of research and the current status of problems in the morale area.

Bernberg (11) finds four items of a group morale questionnaire which predict self-rating of morale. From his report it would appear that the data are available to cross-validate these four items. Perhaps this will be done later. He reports, however, that his earlier work showed no relationship between his morale indicators and a variety of criteria.

Leadership.—Fleishman (33) gave a Leadership Opinion Questionnaire to 100 foremen. This was scored on two major dimensions: "consideration" and "initiating structure." The questionnaire was revised and 20 items for each dimension were selected. The reliability of each set of questions is satisfactory, and there is a low correlation between the two dimensions. The author reports that people higher in the plant hierarchy have less "consideration" and felt the worker should have more "structuring." Although the data are limited, this study reports that "departments with high worker grievance rates contained foremen who perceived their own supervisors as expecting them to lead with a lower degree of consideration and a higher degree of structuring."

Moore & Smith (72) consider the problem of noncommissioned officer leadership as determined from interviews and a questionnaire. They find the following needs: NCO leader training; distinction in rank between supervisors and technicians; more backing of authority; examination of selection procedures for NCOs; and a study of conflict between leadership ideologies. They discuss the findings that the concept of good leadership is dependent on one's position in the leadership hierarchy. This would seem to fit in with Fleishman's (33) conclusions.

When one considers studies of leadership in conjunction with those of job satisfaction, it should be possible to reach some understanding of the factors involved in morale. Unfortunately, many of the workers in this field have taken one approach or another, and the results are not easily reconciled. Perhaps this confusion springs from the absence of common (or any) criteria in this area.

Communications.—A factor which combines the problems of job satis-

faction and leadership is communications. Much has been done in this area in the laboratory, but we will discuss only those studies directly related to industrial situations.

Davis (26) describes a method of communication analysis which investigates the variables of timing, media, subject matter, and level. This technique is called ECCO analysis. The method follows a particular piece of information from start to finish. Davis explains this technique and reports that it is reliable and valid for business use. He feels it is especially useful for comparing communications between department branches or entire companies. This article also briefly describes other types of communication analysis.

In written communications to employees it is a truism that the writings should be at a readable level. MacKinney & Jenkins (66) analyzed 400 employee-written letters taken from the General Motors "My Job Contest." This analysis was made to determine whether or not readability was related to job level. Using the Flesch RE formula they find a relationship between reading ease and job level. The authors point out that this is a measure of writing level, and there is no clear-cut evidence that this is the same as reading level. If there is a high relationship, it would follow that job level should determine in part the level of written plant communications.

That systems of communication can produce problems in industry is brought out by Campbell (17). He reports that when workers and managers have conferences, leaving supervisors out, the supervisor feels he is bypassed and lacks information concerning what occurred in the conference, thus losing status. The supervisors apparently accepted the principles involved in the consultation but did not approve of the inadequacy of the communication between management and themselves. Campbell says this affected their attitude and other aspects of their behavior, but gives no evidence for the latter.

In spite of the fact that communications are of great importance, Stryker (98) points out that there can be "too much of a good thing." Overcommunications can lead to what Stryker calls "the frustration of knowing too much." This was reported as the case in a relatively small manufacturing company, where quite complete communications resulted in everyone worrying about everyone else's problems. This does not mean that free communications should be condemned. Perhaps with better delegation of responsibility and a second thought as to what should be communicated this situation of overcommunication would not occur. Hovland *et al.* (49) have recently published a book which although not directly related to the industrial situation gives a very good review and analysis of factors affecting credibility and effectiveness of communications.

TRAINING

Closely allied to studies of organizational effectiveness are those reporting on leadership training.

Fleishman (34) studied four groups of foremen. One group did not re-

ceive leadership training; the other three groups did receive the training for varying lengths of time previous to the study. All groups were tested on a leadership questionnaire, a questionnaire concerning how the foreman's own boss should supervise, and a third questionnaire concerning what the foreman's boss expects of him. Workers under and supervisors over the foremen were also tested. On the basis of the foremen's supervisors the groups were split into different leadership climates. One finding was that the leadership climate was related to the manner in which the foremen lead. Those who worked under supervisors who were "considerate" were themselves more considerate. The results show an increase in "consideration" after completing the course, but this apparently does not last very long when they are back in the plant situation. As the author states, this emphasizes the dangers of evaluating a training course of this type immediately after the training is completed.

Hazeltine & Berra (46) conducted an attitude survey on 200 members of a supervisory group in one plant. The results were analyzed into 10 major training areas such as: training in supervisory techniques, better communications, etc. After the survey was completed training programs and meetings were held on these various areas. One year later the survey was repeated and some improvement was noted in 25 out of 64 possible items. It is unfortunate that a control group was not used to evaluate the significance of this improvement. Again it should be noted that this study may indicate that the training program changed supervisors' attitudes but shows nothing concerning the improvement in supervision. Form & Form (35) also report on some unanticipated results of a foreman training program. Here they feel the change, a deterioration in "human relations," was probably attributable to a change in the objectives of the course. This consisted of switching from training to indoctrination of a "captive" audience. Other reasons given for the unexpected results of this training involve such things as poor union clearance and poor recruitment for the training.

Klippert & Clay (57) report using the case method of teaching management development. Unfortunately, the only evaluation of the course consists of comments by the "students" which gives us little insight into the usefulness of this type of management development training. Lindbom (64) also evaluated a course in supervision by asking students a series of questions. He finds that a "typical comment" is that the supervisors who had had the course said they showed more consideration of the employees' problems. This may seem contradictory to Fleishman's (34) study. However, when we consider the differences in techniques used to evaluate the course, this is not surprising.

Tannenbaum, Kallejian & Weschler (99) describe a training program which they call "vertically structured." This consists of having all managers of a given organizational unit present. This is an attempt to give the "chain of command" a chance to understand each other's problems in the training situation with the hope that this understanding will carry over to the work

situation. The authors state that "Our experience indicates that working together on mutual problems of interpersonal relations produces a more cohesive working team." We cannot tell if they mean they have actually experimentally validated this conclusion or if this is a casual observation. Crow (23) seems to recommend group training in a horizontally structured situation. He wants to train groups of common needs. In this article there is a brief discussion of conference technique, case study, and role playing.

Allied to group training of leadership is a study by Torrance (107) who investigated various methods of critiques on group problem solving. He used (a) unstructured nonauthoritarian critique, (b) directive critique, (c) no critique, (d) self-critique (in which the experimenter left the group), and (e) structured nonauthoritarian critique. Using ratings on problem solving and improvement in problem solving as criteria, Torrance shows that the groups critiqued according to the more highly structured methods showed the greater improvement. Groups participating in the unstructured nonauthoritarian and self-critique do not perform significantly better than the control group. This study is interesting and is in some ways related to the early work of Bavelas & Barrett (3) who find that communications and problem solving are faster in groups who work in more highly structured situations. However, the morale of this group is not as high as that in the groups which perform more slowly but which are less highly structured. It would be interesting to see whether morale measures in Torrance's situation varied according to the kind of critique technique used. It would also be interesting to see if there were any differences in flexibility of problem-solving behavior using the different critique methods.

A study dealing with conference training done on life insurance agents is reported by Baxter, Taaffe & Hughes (4). One main purpose of the study was to compare centralized schools versus training using the same text and assignments under local supervision. As has been indicated previously, this is an example of the use of multiple performance criteria. The results show that there were no significant differences on any of the criteria measures at six months and one year later. These authors conclude that the cost of the conferences did not seem justified. It should be pointed out that interview impressions of men immediately following the training "suggested that the conference trained group had greater enthusiasm and eagerness for undertaking the new job." These effects were apparently short lived. Fortunately, the investigators did not stop with interview impressions.

Wallace & Twichell (114) attempted to evaluate insurance agent training as carried on at one of the campus training centers. Agents attending campus training were compared with those not attending but having whatever training was afforded by their companies. The men were individually matched on pre-school production, marital status, age at time of contract, and Aptitude Index score. The results showed that the campus trained men produced more in the follow-up period and survived longer. The greatest gain occurred for men who entered the course early in their careers. There was no attempt

to track down the cause for this difference. However, the fact that matched groups were used and a fairly reliable criterion for evaluation was employed does give evidence that one kind of training for life insurance agents is better than others. Such evidence is not often available in other industries, and the authors point out that a methodology of this kind is required to evaluate improvements in training programs. An examination of the unique characteristics of the campus course provides some hypotheses concerning the variables which may be related to training effectiveness. Another study on the effect of various types of training on performance is reported by Fattu & Mech (29). Using a control group and two experimental groups having different instructions, they report that a lecture on basic knowledge plus one on "trouble shooting" is superior for training on the location of malfunctions to lectures on basic knowledge plus machine function.

EQUIPMENT DESIGN

Since one of the goals in training is the efficient use of equipment, we might think of equipment design as related to the training area; here too the aim is efficient use of equipment. However, good machine design should not only consider ease of learning but also the conditions imposed on an operator after he has learned the task. For example, a machine might be designed so that the operation was extremely simple, so simple in fact that a work decrement may occur. It should be remembered that maintenance of the machine should also be considered. A machine may be "human engineered" so that it reduces the complexity for the operator but intensifies the maintenance problem.

One area of equipment design has been given a good deal of attention. This is the field of display. Much of this work has attacked the problem of dial reading. The early work in this area is reported in AAF report 19 (31). Ross, Ray & Della Valle (82) have continued this work in evaluating the effect of pointer location in dial reading accuracy. Using tachistoscopic presentation of three dials (upper-half dial, lower-half dial, complete circular dial), these authors report that dial shape, and quadrant in which the pointer was located were not important variables. However, for the circular dial fewer errors occurred at the 9, 12, 3, and 6 o'clock positions. Another display problem concerning the effect of pointer alignment in a group of dials was studied by Johnsgard (54). Again using tachistoscopic exposure of simulated banks of dials, Johnsgard finds that presentations using pointer symmetry give check readings of equal accuracy to those using uniform alignment; one of the dial groups with symmetrical arrangement was even superior. The author also suggests that check reading improves with practice.

White, Warrick & Grether (117) find that for check reading a group of dials, pointers aligned at positions 9, 12, 3, or 6 o'clock show little difference. Using a variety of pointer shapes they found little improvement in check reading over the conventional pointer design. Cohen, Vanderplas & White

(20) investigated the effect of viewing angle on dial reading. They find that as viewing angle decreases, keeping parallax constant, the errors increase. They also report that parallax affects performance similarly to viewing angle. Both seem to follow a function proportional to the cosecant of the viewing angle.

Another study dealing with flying is that by Murray (76). He was interested in devising techniques to evaluate charts so that principles could be developed for chart construction. Using air craft navigational charts he developed a readability test and a preference test. Certain general principles of good chart design are described.

Lincoln (63) describes a study of visual tracking. This is a common task in industry as well as in aviation. Using three kinds of tracking: direct, aided, and velocity, he finds differences in initial performance as well as differences in transfer of skill among the three types. Direct tracking, as might be expected, is superior for complicated target courses. Aided control is superior to velocity control. This is the type of study which tries to evolve general principles for equipment design. It is hoped that sometime in the near future work in this area will have reached the point where generalization can be made concerning optimal design of new machines. Certainly that is the direction in which psychologists in this area are going, and a great deal has apparently been accomplished toward this end.

JOB ANALYSIS, DESCRIPTION, AND EVALUATION

Job analysis and description, particularly as they relate to job evaluation, are discussed in a thorough review of the literature on technical (sub-professional) jobs (55). Jones, Hulbert & Haase, after examining 307 articles, conclude that the definition, description, and analysis of technical jobs involve special difficulties and deserve more thorough study. They also point to two disturbing trends in the literature. "One is the prevalent idea that a good job description can be used for any purpose—wage evaluation, selection, training, study of occupational families, etc. Such might be the case if good meant exhaustive but in practice it does not." The second trend they mention, which must have already disturbed those who deal with the practical problem of job analysis, description, and evaluation is the growing tendency to pervert job descriptions for the purposes of wage justification. This is magnified by the recent emphasis upon abbreviated job evaluation scales.

The difficulties inherent in the evaluation of research technicians are discussed with considerable insight by Steele (95). Particular attention is given to the problem of assigning proper weights to the value of supervisory or administrative skill and of individual research creativity. A third indication that the industrial psychologist's increased self-awareness is leading him to worry about the description and analysis of his own and allied jobs, is found in a paper by Williams & Davis (120). These authors polled a number of professional personnel directors concerning the characteristics regarded as

desirable for professional personnel directors. They find considerable disagreement and some curious omissions ("intelligence was not mentioned at all"). The study may or may not mean something more than that personnel workers don't know what kind of people they should be.

A study along more conventional lines is reported by Nadler (77). He describes a time sample method ("occurrence study") for obtaining job descriptions of assistant buyers in a department store and claims a number of advantages for it, mainly, that it covers a longer period of time and thus obtains a more representative sample of individual activity. The procedure is ingenious, although some may doubt that the author can meet his requirement that "the individuals being studied will pay no particular attention to the analyst." Nadler discusses the usefulness of the procedure in providing data for work simplification, for individual training based on factual knowledge of what the individual actually does, for determining areas where further training is needed, and for redefining jobs and reassigning personnel.

A practical use of job descriptions for detecting overlapping job duties or unassigned responsibilities is described by Bellows & Estep (7). A procedure of "shred-out" and job resynthesis based on the examination of job descriptions and three proposed organization charts was employed and resulted in a reduction of the number of managers required. Most of the gain apparently came from the reassignment of clerical and routine duties to lower level personnel.

Guion (41) attacks the important problem of supervisors' employee load by attempting to identify factors in the job situation which are associated with varying employee load levels. He finds nine factors which are associated at the 1 per cent level and two at the 5 per cent level of significance. Regression analysis was employed to combine the variables, and the resulting predictions were found to have an r of .64 with actual loads. The author does not offer a conjecture on how much this might shrink if cross-validated.

The analysis of jobs in relation to the effects of fatigue is reported by Davis & Josselyn (27). Two semiskilled, female manual workers were studied with photographic and time-recording equipment. Examination of the average daily productions during periods preceding and following the study showed that they did not differ significantly from the average daily production achieved while the study was being made. (The similarity to the Hawthorne study appears to lie only in the smallness of the sample.) Definite work decrement was found for both operators in terms of the total number of devices completed in a unit time. However, there was no apparent decrement in "effective operation time" defined as the time spent exclusively in completing an item or unit of activity, excluding all stoppages or delays. The delays comprised 30 per cent of the available working time. Eighty per cent of the delay was called "personal" while only 20 per cent could be attributed to equipment or other operational stoppages. Both types of delays increased markedly in the late afternoon. The author concludes that the operators use the same work method at a constant level of efficiency throughout the day,

when they work. Work decrement is therefore seen as an increasing reluctance to perform the operations rather than as a breakdown in method or an increase in errors. The difference between these results and those reported from laboratory studies is attributed to the high and constant motivational level of subjects in the latter. The relation of these results to Smith's (91) is apparent. However, her inability to find a characteristic work curve for individuals or groups emphasizes the fact that conclusions based on two workers are highly suspect. This study deserves repetition.

A report in *Time* magazine (1) should be mentioned in passing. A large corporation is reported as having discovered that "job enlargement" and "job rotation" as opposed to assembly line specialization produces fewer feelings of monotony and fatigue, fewer absences, and fewer errors. Another study is reported in which a large company introduced "job enlargement" into its billing department and experienced a drop in overtime of 50 per cent. Intrigued, the company polled 122 others and found a positive relation between billing costs per customers and the degree of job specialization within the department.

Smader & Smith (88) and Harris & Smith (44) continue their series of reports on the dimensional analysis of motion. They should bring new life into the field of time-motion studies and offer new hope to those who have been fascinated with the possibility of "pure" psychomotor tests.

Although somewhat remotely related to the area of job analysis, mention should be made of studies of the suitability of special groups for various jobs. Smith (90), on the basis of supervisors' evaluations, finds few characteristics of ability or of personality which show a change with age in skilled, unskilled, or clerical groups. Speed and ability to learn show some loss, but the oldsters receive as high ratings on efficiency as the younger men. "Slow but steady" seems to be the descriptive phrase. One wonders if a process of natural selection has occurred here. Are the older men in this study those who were retained in their jobs because they were unusually capable?

Belbin (6) investigated the difficulties experienced by aging male and female workers. He found a definite tendency for both the men and women to transfer from operations which require continuous bodily movement and activity. This effect was considerably enhanced when the workers were also paced by a machine or conveyor. The female workers showed very early (mid-twenties) difficulties in "time stress" jobs, particularly when the jobs also required a relatively high level of sensorimotor skill. A comprehensive and thoughtful treatment of the aging worker problem is presented by Breckinridge (12).

Thiele (102) studied a group of "educationally blind" and examined the jobs which they were able to hold. He found significant relationships between certain of their characteristics such as "verbal ability," "dependability," etc., as rated by an interviewer, and their occupational achievement as judged by four persons closely acquainted with problems of the blind.

Hausrath (45) employed demographic analyses, opinion and attitude

surveys, content analyses, critical incident technique, and sociological community surveys in studying the effect of racial segregation in the Army. His results demonstrate that the integration of Negro and white soldiers allows more effective use of man power through more even distribution of aptitudes, leads to satisfactory unit performance, and, with time, produces a reduction of resistance to desegregation.

SELECTION

General.—Selection continues as the subject most frequently represented in the literature and, probably, as the area in which the industrial psychologist enjoys his greatest prestige. Spriegel & Dale (93) find that almost all of a group of 600 companies ("generally recognized as having well-developed personnel policies") use an application blank and a selection interview. Seventy-five per cent use selection tests of some kind. In comparing the results of their most recent survey (1952) with similar ones made earlier, the authors find a decided trend toward tests, particularly those of the "mental (intelligence)" type. This finding will surprise some readers who have supposed that the increased emphases on the specificity of test validity to populations and job situations would imply a trend toward tests of the "pure" type, or, at least, of the job-oriented variety.

A somewhat novel approach to the specificity of selection problems is made by Mandell (68). This author lists and discusses a number of factors in the "organizational environment" which might affect the hypotheses entertained by one who was interested in developing selection standards or methods. Emphasis is placed on the possibility that job analysis, when limited to a statement of individual tasks and duties, fails to provide information needed to make a predictor realistic in terms of the suitability of an applicant for the specific organizational environment in which the job is to be performed. This position that predictor development should take over-all organizational environment into account is one which most will agree with but few will do anything about. Its implications for both predictor construction and validation design are obvious and important.

The proper and meaningful interpretation of selection validation data has interested a number of writers. This reflects a growing belief that coefficients of correlation tell only part of the story, even when employed on those rare sets of data which fulfill the underlying assumptions.

Jenkins (53) shows that the percentage improvement over chance in accepting the top quarter of workers and rejecting the bottom quarter will be approximately equal to the validity coefficient of the predictor when the selection ratio lies between .33 and .67. Brown & Ghiselli (15) consider the percentage increase in population proficiency resulting from the use of predictors with varying validities, under different selection ratios, and with differing degrees of homogeneity in criterion performance. This is probably the most comprehensive treatment yet given to this problem. It is also healthy in that it shows how limited the gains from selection must be. For example,

the use of a predictor with a validity of .50, with a selection ratio of 10 per cent, and a ratio of standard deviation of raw criterion scores to the mean equal to .25, can be expected to select a population whose criterion performance will be 23 per cent better than that of an unselected group. The authors would be the first to agree that such an interpretation involves a number of assumptions about stability of empirical validities, effect of turnover upon over-all performance level, etc. However, an analysis of this kind is most useful in showing that the bald statement of a predictor's validity fails to give information necessary to evaluate its potential contribution to a specific user.

Kashdan (56) has considered the usefulness of a predictor when employed to select the better of two workers. His point of departure is the recognition that the selection decision is rarely made on the basis of validated tests alone. Other sources of information are usually used and may actually throw the decision against the predictor indication. The personnel director is therefore in need of a method for estimating the risk incurred in making such a decision. Given a validity estimate based on a product moment coefficient derived from appropriate data and test scores in standard form, an expression can be obtained of the probability that the worker with the higher score will exceed the other with respect to criterion performance. As Kashdan points out, this method of evaluation seems well adapted for interpretation to management.

Doppelt & Bennett (28) provide an example of evaluating selection procedures in terms of the savings effected in training costs. Given a predictor validity, the distribution of predictor scores, the proportion of successes at each score level, the desired number of successfully trained workers, and the cost of training and testing each worker, it is possible to construct curves showing the cost per successful worker for various selection cut-offs along the predictor scale. Even when the cost of the selection procedure is very low in relation to training cost, the most efficient cut-off is likely to be unrealistic because of the large number of applicants to be processed. However, the loss in effectiveness produced by lowering the cut-off may be computed. While the method is useful and certainly easily interpretable to management, its users should be careful to take into account the cost of recruiting, i.e., of feeding applicants into the selection procedure. This cost may vary from industry to industry, company to company, place to place, and year to year. In some situations, it may greatly exceed the cost of applying selection procedures.

Taylor & Nevis (100) report an interesting if only suggestive study on the validity of a selection procedure after it has been placed in operation. In the process of performing a follow-up validation of a procedure for selecting salesmen, they found that omissions of one or more of the selection devices had occurred in a considerable proportion of the cases. A further analysis was performed for each of the predictors to determine the relation between omission of the device during selection and subsequent job termination.

It was found that "every test or form was used less often in selecting men who proved to be poor risks tenure-wise." It was also found that a significant relation existed between termination and the number of predictors omitted. Not surprisingly, the more difficult of the procedures to use (an Interview Guide and the Strong Vocational Interest Test) were more often omitted, and their omission proved to have high relative predictiveness.

The authors present some ingenious hypotheses to account for these findings (although neglecting the possibility that branch managers who do not follow directions in administering selection tests are also less effective as trainers and supervisors). However, they quite properly regard this study as illustrating the decided need for ensuring against the deterioration of a validated selection process after it has been installed. The reviewers would like to re-emphasize the point. The fact is that our systematic knowledge of the relationship between experimental or empirical validity and validity for actual operational conditions is woefully inadequate. The problem is not adequately covered by continuing to determine validity coefficients and making corrections for restriction of range. In many industrial situations it is possible to identify factors which, through a period of operational use, may serve to obscure "true" validity. It is also possible to recognize factors which will lead to considerable overestimation. Changes in the criterion may occur with unpredictable and puzzling effects. The need for greater attention to this problem is acute. From the viewpoint of management, the statement of "trial-run" empirical validity is only evidence that the predictor is worth a try operationally. It would seem, as Taylor and Nevis point out, that the industrial psychologist has not fulfilled his responsibility until he has developed methods of determining what his procedures accomplish when they are made a regular part of operations.

Tests.—Brown & Ghiselli (14) have made a courageous attempt to integrate the many reports of test validities into some generalizations about what is valid for what. They considered only studies which employed criteria of job proficiency (production record, job performance ratings, etc.) and aptitude tests that have enjoyed fairly common use. The tests were classified under 18 rubrics and the jobs under 21. Using over 3500 validity coefficients, they obtained mean estimates of validity for each type of test applied to each type of job. A cluster analysis of the matrix of correlations between tests where their validity coefficients were used as scores and the jobs as individuals, produced three major test groups: "intellectual, spatial, and motor." A score was then derived for each of the job types indicating its relative amenability to prediction by the test groups. The results confirm some generally accepted notions of relationship between tests and jobs but provide enough surprises to make them of considerable interest. The authors also point out the potential usefulness of such data in developing classification procedures where differential rather than absolute validity is the major requirement. This attempt to escape from the sterile field of complete em-

piricism must enlist everyone's sympathy and admiration. One shares with the authors the wish that the data upon which the study was based were more deserving of the care and energy expended upon them.

Flanagan's Aptitude Classification Test battery is now available (32). It includes 14 tests, and the manual describes 18 jobs for which the use of the tests in screening is recommended. Validities are presented for 10 occupational fields where rate of progress of high school graduates was employed as the criterion. Unfortunately, for all but two occupations, the number of cases is very small. Some readers may feel that the validity estimates (especially those based on multiple correlation) are quite optimistic. Extensions of the validation studies are undoubtedly planned.

Considerable attention has been given to the usefulness of short tests. Bennett & Gelinck (9) report on the development of three 5 minute tests (Short Employment Test Battery) designed to test "verbal intelligence," "numerical skill," and "clerical speed and accuracy." Despite the tests' brevity, quite satisfactory reliability was obtained. The intercorrelations of the tests were low while a score based on all three was highly correlated with score on the longer General Clerical Tests. Validities for stenographic learning or status are encouraging although the authors point out that they are preliminary. This battery was subsequently studied by Wilkinson (119) who administered it (in two forms which were shown to give comparable results and were then combined) to 177 clerical workers who had been employed for a minimum of somewhat more than a year. Criteria used were job level (salary classification) and the average of at least three regular semiannual merit ratings (only employees who each received three or more ratings which were "very nearly the same" were included). The tests show substantial correlations with job level. The arithmetic test had the highest validity (.49) which was only slightly exceeded by the validity of a combination of all three. Wilkinson concludes that no meaningful relationships were found between the criterion ratings and scores on any of the three tests, but his table shows correlations between ratings and arithmetic of .21 and ratings and clerical aptitude of .23. Considering the possible sources of unreliability in the ratings, some may feel that the author's standard for meaningful relationship is somewhat stringent. However, the need for further investigation of the Short Employment Test Battery in relation to performance criteria is apparent.

Tomlinson & Preston (105) developed a short test designed to determine the probability of achieving a qualifying score on a test battery weighted for prediction of success in one group of Air Force training schools. A small sample (3 to 6) of the items in each of the tests of the battery was selected on the basis of several criteria. Items were included which had higher correlation with the aggregate score, higher correlation with the parent test, lower discrimination for other tests of the aptitude index composite than for the parent test, and difficulty index centering around 60 per cent right (uncor-

rected). The resulting 30-item test gave a correlation with the aggregate weighted score of .86 in an independent sample of 882. The authors warn that the technique should be applied only when dealing with reliable, well-validated selection instruments.

A growing impatience with the long and tedious interest inventory tests is exemplified by Clark & Gee (19). They show that, just as might be expected from a consideration of multiple regression, an optimum number of interest items can be found for discrimination between a specific group (electricians) and "trades-men-in-general." This number is very considerably less (43 out of 580) than that used in the Strong Vocational Interest Blank. The authors indicate that the reduction in number of items produced a serious loss in reliability as determined by part-test procedures but reassure themselves with the finding that no appreciable loss is found in test-retest reliability where the intervening period is relatively short. These results were obtained when the items were scored with unit weights rather than the more elaborate weighting system employed by Strong. The authors proceed to give evidence that the simpler procedure produces as good if not better separation of groups. Finally, applying either an iterative or an internal analysis method to increase the heterogeneity of scoring key content was found to increase validity, although the loss in reliability is sufficient to indicate a need for caution. Considering the popularity of interest inventories, it would appear that Clark & Gee have provided a basis for the saving of many millions of man hours. However, Strong (96) discusses these results, emphasizes the importance of long term test-retest reliability for most uses of interest tests, and provides data to indicate that considerable relation exists between odd-even and test-retest reliability where the latter involves an interim period of 18 years.

Mosel (74) takes the final plunge and presents a fascinating defense of single item tests for personnel screening. Some encouraging empirical data on the relation between single vocabulary items and job performance ratings of 100 assembly workers are presented. However, it is the obvious zest with which the author considers some of the implications of single item tests for interpretations of validity, reliability, factor content, and cut-off selection that makes this paper both readable and significant.

Perhaps the most frightening of the trends in testing is the growing tendency to accept the trait names assigned to test scores as meaningful and having an *a priori* relationship to everyday life. Counselors, industrial "evaluators," and others of the clinical persuasion need badly to have their attention directed to studies of the type reported by Iscoe & Lucier (51). These authors administered the 1951 revision of the Allport Vernon Scale and the Kuder Preference Record (Personal) to 90 adult males. On the basis of the definitions of traits given in the test manuals, they hypothesized that a high correlation should exist between the Theoretical score on both tests, between the Economic score of the Allport Vernon and the Practical score of the Kuder, and between the Political score of the Allport Vernon and the

Sociable and Dominant scores of the Kuder. They also predicted a negative correlation between Allport Vernon Aesthetic and Kuder Theoretical. The correlations obtained in no way supported any of the hypotheses. For example, Theoretical correlated .20 with Theoretical. The highest correlation (.47) was that between Aesthetic and Theoretical in a positive rather than the hypothesized negative direction. When we consider the widespread use of these instruments and the advice freely based upon them, we are likely to feel that it is time for a little soul-searching.

Validity data have been reported for performance criteria of such widely varied groups as bank clerks, taxicab drivers, sewing machine operators, and weathermen. Seashore (87) reports four studies of the validity of clerical tests for the prediction of training performance of bank personnel. A thoughtful discussion of the sampling and criterion difficulties encountered in performing validation studies in actual business situations is also presented. Brown & Ghiselli (16) continue their interest in taxicab drivers and report some validity coefficients between several tests and a criterion based on drivers' sales. Their data provide a good example of the fact that a priori judgements about what tests will or will not predict a particular criterion are risky at best. As the authors point out, few investigators would have supposed that tests of dotting, tapping, or discrimination of distances would be predictive of a complex criterion of the sort used in this study.

Walker (113) applied a standard battery of industrial vision tests to a group of female sewing machine operators. Here, logical analysis had indicated a high likelihood that visual acuity was a requirement for successful performance. No significant relationship was found between any of the tests and performance. The after the fact hypothesis then became that "the women could sew sheets with their eyes closed." The way of the theoretician in these fields is a hard one indeed!

Jenkins (52) used an ingeniously constructed criterion in a study of 92 Air Force weather forecasters. His study is unique in that the criterion, an objective performance measure, had a corrected part-test reliability of .90. One test (the Names section of the Minnesota Clerical Test) bore a significant relation to performance. Of further interest is the fact that another rarity occurred. Two other tests were found to show promise as suppressor variables.

The prediction of turnover has received attention from Tiffin & Phelan (104) and Kriedt & Gadel (58). The former study involved an item analysis of the Kuder Preference Record (Vocational Form C-H) and the construction and cross-validation of a tenure scale. The original analysis was based on over 1000 cases and the cross-validation on 487. A significant discrimination between short tenure employees and others was obtained. Kriedt & Gadel also studied the 3 month and 12 month turnover of a group of female clerical employees. Four predictors (General Ability Tests, Clerical Speed Tests, Biographical Data Blank, and Interest Questionnaire) were cross-validated on a sample of 358 and found to have a validity of .40 for 3 months turnover and .33 for 12 month. Biographical Data has much more weight than the

other predictors in both regression equations. The authors point out that, while their results are in accord with the frequently stressed negative relationship between intelligence and the likelihood of staying on a routine clerical job, their study also indicates that other factors such as family and educational background and interests tend to be considerably more important.

Accident records continue to be used as selection criteria. Parker (79) examined the validity of a number of psychological and vision tests, personal history data, and some physiological measures for predicting preventable and nonpreventable accidents per 5000 miles for 104 truck drivers. Preventable accidents were related to three scores on the MMPI and one score on the Kuder (Wherry-Doolittle shrunken multiple correlation of .36). Nonpreventable accidents showed no relation to any of the psychological test data but were related to visual test performance and to marital status.

Speroff (92) having already found that steel mill workers who were least liked by their fellow workers tended to have relatively high accident rates, hypothesized that such workers "lack the perceptual and/or motor skills to handle either work dexterously or people diplomatically." This led to the consequence that workers with low popularity and high accident rates should obtain lower scores on the Kerr-Speroff Empathy Test than those with high popularity and low accident rates. Twenty men in each category were selected and tested. It is stated that a significant difference in scores was found in the predicted direction. However, examination of the data table suggests that an error in the placement of the decimal point in the critical ratio has been made and that the magnitude of the difference is actually in accord with the null hypothesis.

Van Zelst (110) examined the relationship between Empathy Test scores and four criteria: interpersonal desirability, How Supervise? score, job satisfaction (Tear Ballot for Industry Score), and a self-judgement score. Using a sample of 124 skilled male workers, substantial correlations were found between the test scores and each of the criteria. The author points to a previous finding (based on 32 cases) that the validity of the Empathy Test for the merit rankings of sales managers is .71 in suggesting the inclusion of the Empathy Test in batteries undergoing validation for the selection of leaders, sales personnel, and counselors. Weitz & Nuckols (115) found that the responses of life insurance managers on the How Supervise? test were not related to the termination rate or performance of their subordinates. A relationship was found between test score and education.

Gilliland & Newman (38) report a study on the Humm-Wadsworth Temperament Scale as a device to detect "problem employees." They studied a group of 405 white collar workers nine years after they had been tested. The scores were evaluated in accord with a method believed to be equivalent to one described earlier by Humm. The men were then classified into 3 groups: the 191 who were still employed and rated as "successful" or "satisfactory," the 139 who had left the job but without an unfavorable service record, and the 75 who had been dismissed or had resigned while on proba-

tion. The test produced only chance differentiation between the worker groups. Humm & Humm (50) retort that it was not really their method of appraisal of applicants that was used and that the criterion is probably oversimplified anyway. It is difficult to perform a validity study acceptable to these authors.

Generally negative findings for the validity of descriptive "personality" tests in industrial situations appear to have had little deterrent effect upon many writers. Thus, Corona (21) informs us that the Rorschach test "is analogous to an X-ray view of the underlying personality structure of an individual" and highly recommends its use in ensuring against the hiring of psychopathic personalities.

The interview.—The ubiquity of interviews and their resistance to experimental and validation procedures remains a source of embarrassment and a generator of frustration. Bellows & Estep (8) contribute a volume entitled *Employment Psychology: The Interview*. This is a praiseworthy attempt to capitalize on what is known about effective interviewing, to caution about what is not known, and to stem the tide of clinical, nonstatistical claims. One cannot blame the authors for turning their discussion from interviews per se to weighted application blanks, personal data sheets, and trade tests. However, the end result is to reconfirm the knowledgeable reader's belief that we have little to say of a positive nature about interviewing or interviewing technique. The authors say well what can be said. That their long experience and wide knowledge does not produce more is a gloomy commentary on the progress made in this area.

Daniel \ddot{s} (24) has applied a factor analysis to data obtained on 54 employment interviews. Measures were made of 14 aspects of interview and interviewee behavior, and five factors were extracted. The factors are thought to be useful in adding to the objectivity of the interview and as a basis for a check-list for interviewers to use in evaluating their own techniques. A number of conclusions about desirable interviewer behavior are presented as resulting from the analysis. Their derivation is not clear to the reviewers. In general, they seem to boil down to the usual platitudes. Interviewers should keep their mouths shut, stick to the point, be permissive, give no advice, and be the kind of people in whom others confide. It may be hoped that interview analysis will prove more fruitful than this early effort would indicate.

Crissy and his co-workers have reported the results of a concerted attack on the interview problem as it relates to the screening of submarine candidates. Their work includes an extensive bibliography (80); examination of the reliability and validity (for a basic school pass-fail criterion) of personal history data (65), and of a technique for determining degree of identification with the job stereotype (22); and an intensive analysis of the content, time patterns, intra- and inter-rater reliability, and validity of the interviews themselves (81). This program is almost certainly the most significant contribution made to the interview area in recent years.

Perhaps the most encouraging study of the period is that reported by

Handyside & Duncan (43). They performed a follow-up study of 44 men who had been part of an experimental group of 110 candidates for promotion to supervisory positions in a large heavy engineering plant studied about 4½ years previously. These men had been evaluated by plant managers in accordance with the customary selection methods of interview and discussion among the managers. The managers had completed a recommendation form on each of the candidates as a kind of systematization of the customary selection methods. The men had also been evaluated by an experimental procedure which included, (a) two intelligence tests (verbal and nonverbal), (b) especially constructed interviews where the interviewers also had access to a biographical data sheet, the intelligence test results, and a recommendation form completed by the candidate's immediate supervisor, (c) a managers' panel review of the information noted above, (d) the ratings given on the basis of observation of behavior in three group discussion sessions (for 40 per cent of the candidates), and (e) a final review of all evidence by the managers and the investigator. The men were classified into 10 criterion categories on the basis of a composite rating which considered their promotion record in combination with performance ratings given in the course of their careers.

The results are dramatic. Managements' normal method of selection is found to have statistically insignificant validity (partially springing from a systematic tendency to prefer older workers). But everything employed in the experimental appraisal worked. Intelligence tests correlated .51 with the criterion! One interviewer's assessment had a validity of .61, another of .55. The panel review results had a validity of .55 and the group discussion ratings gave .58 ($N=28$). The final over-all review of evidence produced a validity coefficient (corrected for range restriction) of .72. The authors estimate that the reliability of the criterion is .65 and of the final grading, .95. Thus, corrected for attenuation, the validity estimate becomes .92! The authors do not determine what could be done by weighting the various scores and ratings for a multiple correlation. The measures have relatively high inter-correlations. The significant thing in this study (neglecting the surprising success of the intelligence test) is the outstanding success of the interviews (the group discussion sessions appeared to produce about the same results as the panel review of the interview gradings). Unfortunately, a detailed description of the interview content and technique is not given in this paper nor does it appear to be available in previous publications. It is certainly to be hoped that such information will be forthcoming. In any case, they point out that their most fruitful area of work will lie in criteria improvement.

Perhaps our review of the literature should end with the last sentence. Certainly throughout this review the inadequacy of criteria is apparent. If we were to conclude anything from this year's work, it is that more effort should be expended toward evaluating procedures, and this of course demands criteria.

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COMPARATIVE PSYCHOLOGY¹

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The recovery movement in the field of comparative psychology has gone forward with unmistakable vigor. In token of this trend, there is a happy addition to the group of periodicals through the founding of the *British Journal of Animal Behaviour*. This slim but stimulating quarterly is published by the Association for the Study of Animal Behaviour and succeeds the highly irregular *Bulletin of Animal Behaviour*. The appearance of the *British Journal* so closely on the heels of *Behaviour*, in view of the current prosperity of the latter and of the older German journals, is another testimonial to the growing importance of European psychology.

From Britain, too, we have an outstanding book. It is an encyclopedic review of the literature on bees, prepared by Ribbands (1) of the Rothamsted Station. The work is unusual in that it is devoted almost exclusively to formal experiments. This feature is apparent if the volume is compared with the new book by Richards (2) on the social insects; the latter comes closer to the typical naturalistic orientation but is nevertheless still rewarding reading. A third English work by Tinbergen (3) on the herring gull is somewhere in between the two; it gives a notably clear expression of the author's conception of relations between field and laboratory work. The latter book is marred only by a foreword by Lorenz, who hails the analysis of gull behavior as the best that we have for any animal. American rat runners will doubtless regard this as somewhat extravagant.

Tinbergen (4) also presents a small volume on social behavior, an outline of the ethological approach to problems in this field. The familiar stickleback and herring gull are much in evidence, as one might expect by now, but a variety of other species also is included. There is nothing new and startling in the book, but the writing is to the point. For novelty, the yearly award clearly goes to a monograph on mule deer prepared by Linsdale & Tomich (5), a summary of several years of study at the Hastings reservation. The work is not experimental, but does contain a large amount of behavioral information. It is, in fact, a beautiful illustration of how much can be learned from observation conducted with a critical attitude.

In the regular journal literature, the customary group of papers devoted to procedures is very much in evidence. A special committee report (6) on problems of nomenclature in studies of chemoreception is particularly welcome. The relative advantages and sources of confusion in terms applied to chemical solutions are discussed; molar specification is recommended for precise work. Simple instructions are given for the preparation of true percentage concentrations for use in studies where fluid intake is measured in grams.

¹ The survey of the literature pertaining to this review was completed in May, 1954.

Where volumetric units of consumption are employed, specification in terms of weight of solute per unit volume of solvent is preferred.

The ever-present problem of housing the largest number of animals in the smallest amount of space has been discussed by Lane-Petter (7). He is particularly concerned with the question of optimal cage size and presents an empirical formula for the dimensions of small animal cages which summarizes conditions prevailing at a group of institutions that have had good luck with their charges.

On the apparatus side, those who have wondered why their "gridless, wireless" Licklider (8) rat shocker turned out also to be shockless can find the answer in an article of Beck, Waterhouse & Runyon (9). These authors reject the notion of shock transmission by capacitance, as Licklider had proposed, and substitute a potential gradient theory. They describe a new grid-wiring arrangement: odd bars are connected to a pulsed high-voltage electrode; even bars, sides, and back of the cage are electrically isolated. The authors state that this arrangement works very well for shocking monkeys and note that the inevitable fecal bolus shorts out only a part of the grid.

Greenhut & Young (10) discuss at length the problems of methodology associated with the use of a modified Russell (11) distance discrimination apparatus. They conclude that the jump-force criterion is inadequate if used by itself. A combined criterion of jump-force and accuracy is suggested, but with no method of weighting. The questions raised are important, for the authors perform an experiment from which they conclude that rats cannot discriminate distance to any marked extent through vision; learning and kinesthetic cues are held to be required. If this conclusion is valid, and it seems to be, any number of contentions with regard to innateness of visual depth are no longer tenable.

Students of bird behavior continue the application of modern acoustic equipment to the study of bird calls. Fish (12), for example, describes a simple array of commercial instruments which will provide time-frequency curves and discusses an application to the analysis of wren songs. Collias & Joos (13) have employed the acoustic spectrograph to identify relevant components in the sound signals of domestic fowl, and with the information obtained in this way showed that it is possible to mimic natural calls with relatively simple artificial substitutes. The spectrograph has also been used by Thorpe (14) to study the song-learning of the chaffinch, and with it he has identified a simple component regarded as innate. Thorpe concludes that refinements of the song must be learned and that in the bird's first spring there is a period when learning is particularly facile. Once acquired, however, the songs are stable. It is thus possible for local variations in song to appear, but Thorpe believes that the innate component prevents the learning of songs of other species.

There are two new devices for the study of activity in rats, one by Campbell (15) and the other by Eayrs (16). The stated features of the Campbell

apparatus are ready adaptation, low inter-unit variability, and inexpensiveness. The Eayrs apparatus is far more complex, and is designed for the automatic extraction of several indices from the records. Both utilize pivot-type stabilimeter cages.

Eayrs (17) has utilized his new recorder to study the diurnal activity of females in stabilimeter cages and in conventional Slonaker-type wheels. He notes the familiar fact, also cited by Campbell, that a long period of adaptation to the wheels is required. Few investigators will be surprised to learn that the measures obtained were not the same: by distance run, the rats were far more active in wheels. Eayrs concludes that wheel activity satisfies some need that is not present when the animal is housed in the other cages, but this is not identified. Perhaps it could account for the observation of Kagan & Berkun (18) that access to a wheel will reinforce a bar pressing response.

The Campbell apparatus makes its debut in a study by Campbell & Sheffield (19). These investigators noted that increased food deprivation does not yield increasing activity scores if rats are kept in the dark and sounds are masked by a fan. Such trends do appear during periods when the light is turned on and the sound screen off. Campbell & Sheffield conclude that starvation does not increase activity, but lowers the threshold for normal stimuli to activity.

The emphatic implication of external stimuli in the control of activity provided by the foregoing study (19) is in sharp contrast with the typical results obtained for comparable phenomena in lower forms. Many cycles have been identified in the invertebrates which seem to go on regardless of changes in the external conditions. Nevertheless, a recent study by Harker (20) on the rhythm of activity in mayfly nymphs nicely illustrates a restriction too often overlooked. She noted, by time sampling, that activity rhythms are maintained by these animals under continuous light, continuous dark, and reversed conditions of illumination. But this is true only for nymphs bred from eggs exposed to the normal light-dark cycle; the rhythms are absent from nymphs bred from eggs kept in continuous light. A single exposure to the light-dark cycle is enough to establish the rhythm, however, and once established it continues regardless of the external conditions.

There is a notable revival of interest in the sensory capacities of insects, particularly in Germany. With regard to vision, Resch (21) and Schöne (22) have investigated the color vision and spectral sensitivity of the backswimmer, *Notonecta glauca*, and the water beetles, *Dysticus marginalis* and *Acilius sulcatus*. To some extent the results are limited by the methods of measurement; both investigators employ optomotor reactions to moving visual fields constructed of gray or colored papers arranged in vertical strips. More convincing data, particularly with regard to color vision, have been obtained electrically. Autrum & Stumpf (23), stimulating the eye of *Calliphora erythrocephala* with heterochromatic flicker apparatus, recorded directly from the organ, and obtained a detailed function for wavelength discrimina-

tion. This shows a neutral point at 580 $m\mu$, and a general form that is reminiscent of Wright's (24) results for the human fovea.

Other varieties of invertebrate sensitivity have not been overlooked. Burke (25), for example, has identified a proprioceptive-vibratory organ in *Carcinas maenas* capable of indicating rate, extent, and direction of movement in the walking leg. Schneider (26) has considered at length the functions of halteres in *Calliphora*, both from a mechanical and sensory standpoint. A general review of olfaction in insects has been prepared by Dethier (27), and Schanz (28) has worked on the organ of smell in the potato beetle, *Leptinotarsa decemlineata*. Finally, Slifer (29) argues that the permeability of the sensory pegs of grasshoppers (Orthoptera, Acridiidae) to water and acid fuchsin dye supports the hypothesis that these structures are chemoreceptors, hygroreceptors, or both.

Armington (30) has asked whether or not the turtle, *Pseudemys*, has a functionally duplex retina. The answer seems to be yes. Although rods are rare in the turtle eye, these animals nevertheless are very sensitive to low levels of illumination as measured by a discrimination procedure. The maximum is placed around 525 $m\mu$. Armington also finds that electroretinograms made with very intense stimuli show a shift of sensitivity toward the longer wavelengths, and he concludes that the turtle possesses both photopic and scotopic mechanisms.

The latest studies of vertebrate color vision are generally in keeping with prevailing opinion as to the probable distribution of this capacity. Positive results for a teleost have been obtained by Hurst (31) in an experiment with the blue gill sunfish, and Quaranta (32) reports that two varieties of giant tortoise (*Testudo*) can learn discriminations based on wavelength. The one discrepancy arises with regard to color vision in cats: here the score is two to one in favor of tradition. Gunter (33) and Meyer, Miles & Ratoosh (34) agree that cats are color blind, as sub-primate mammals are supposed to be, but Buchholtz (35) comes to the diametrically opposite conclusion. The reviewer is not convinced that the latter experiment is sufficiently precise from a methodological standpoint to constitute a serious challenge to the prevailing belief.

A group of experiments devoted to assessments of vertebrate taste and smell includes studies with birds and dogs. The birds are thought to be very poorly equipped in this respect, particularly so far as smell is concerned. This is borne out by the newest reports, although Hamrum (36) has shown that the powerful odor of coumarin can serve as a discriminative cue for the bobwhite quail. Hamrum also found that this bird reacts positively to sucrose solutions, but does not reject HCl solutions until the concentration reaches .4 N. Positive reactions to various sugars have also been noted by Weischer (37) in lizards, parrots, and starlings, but not in siskins or chickens. Further tests showed that while the lizards discriminated between sugars of various kinds, parrots tested with the same substances reacted about equally to all.

The sensitivity of the highly respected canine nose to various fatty acids

has been measured by Neuhaus (38). This investigator describes an olfactometer designed especially for the purpose. The odors were delivered by jets to the vicinity of response boxes baited with sugar, and sensitivity was measured by discriminative performance. The results obtained, when compared to human data, show that the dog's nose can detect these materials in solutions diluted a million times or more beyond our own threshold concentration.

Developments in animal hearing have been linked this past year to the general question of orientation by echolocation. It has become increasingly evident of late that a variety of species have highly developed mechanisms of the sort initially described in bats. Recent trends in this field are summarized by Griffin (39) in an absorbing review. One of the latest findings is that sounding is utilized by bats for other things than obstacle avoidance. Griffin (40) has recorded the cries of *Eptesicus fuscus* and noted initially that characteristic changes occur whenever the animals dive for prey. Pursuing the observation, Griffin found that the bats would chase cotton pellets sent their way with a blowgun, emitting all the while very short cries that are frequency modulated. Apparently the bat does not depend upon the sound from an insect's wings, as has been proposed, but upon reflections from sounds that it generates itself.

In another experiment, Griffin (41) has shown that echolocation is utilized by the oil bird, *Steatornis*, in finding its way through dark caves. It was first established, by exposure of photographic film for several minutes, that the nesting and roosting sites were in complete darkness. Next, clicking sounds made by the birds while flying out at night were observed to disappear whenever light was available. Finally, Griffin caught some of the birds and plugged their ears as in the bat experiments. The result was the same, for the birds became completely disorientated.

The porpoise, *Tursiops truncatus*, is the newest candidate for membership in this group. Kellogg (42) has shown that the species is well equipped from an auditory standpoint; he places their upper limit between 50 and 80 kc., depending upon the background noise. Kellogg, Kohler & Morris (43) have observed further that porpoises make the requisite sounds. Noted in particular are frequency-modulated whistles in the range between 7 and 15 kc., which occur almost continuously, and rapidly repeated clicks and clacks with much higher components. The authors indicate the next step in the analysis, deafening the animals, but this has not as yet been completed.

While the emphasis has been upon the ear in studies of short-range orientation, the recent work on long-range homing and navigation has been placing the stress upon the eye. There is a distinct trend away from the recently popular magnetic receptor and Coriolis theories, and toward elucidation of the possible role of the sun as a point of navigational reference. The latter hypothesis is being subjected to extensive investigation by Matthews in England and a group led by Kramer in Germany.

The latest report from the Kramer group is a study by Saint Paul (44) which shows that night migrant birds (the warbler, *Sylvia nisoria*, and the

shrike, *Lanius collurio*) have a mechanism for sun orientation like that which has been demonstrated for pigeons and starlings. A training cage of circular plan was used by Saint Paul for this experiment; the birds first learned to go to one of several containers about its circumference to obtain food. This training was accomplished at a particular time of day, and tests were made after changes in the sun's position to see if the birds retained their bearings. At this time the birds showed a tendency to err in the direction of maintaining a constant light angle, but there was evidence for an absolute orientation that must involve allowances for changes in azimuth. That this orientation in fact depends upon the sun was demonstrated by substitution tests and mirror alterations.

The recent work of Matthews (45) in this field is distinguished by the high quality of the statistical treatment; this is a most welcome change in the tradition. Matthews has been primarily concerned with homing behavior, and has emphasized that the initial orientation and subsequent homing are two distinct events. Thus he reports that orientation toward the loft is observed in young and untrained pigeons even though such birds home poorly. Practice, then, is thought to affect the return rather than the determination of bearing.

In another paper Matthews (46) states that pigeons orient poorly if the sky is overcast; they scatter randomly when released under these conditions. This is taken as further evidence for the importance of the sun; homing is much better when the sky is clear. The same relations hold for the Manx shearwater *Puffinus* (47), a bird whose status as a navigator is attested by Mazzeo's (48) account of one that homed from Boston to its colony off Wales in less than 13 days. Matthews proposes that homing birds determine latitude on the basis of the sun's altitude and longitude on a time basis, much as a human navigator might, but without the aid of outside instruments. Griffin (41) speculates in this connection that the pecten of the avian eye may be critically important for the computation of altitude by providing a frame of reference against which the position of the sun can be gauged.

The most dramatic instances of long range navigation are found in association with seasonal migration. Two problems arise: orientation is one; the other is initiation. We are as yet without an adequate theory to account for initiation of migratory flight, but the analysis of the problem continues. During the last year, Wolfson (49) has studied the effects of changes in day length upon the gonadal, fat, and molting cycles of the junco and the migratory white-crowned sparrow, *Zonotrichia leucophrys gambelii*. He concludes that the entire annual cycle is regulated in this fashion and has shown that the cycle can be repeated several times a year by appropriate manipulations of light. Of interest in this connection is the recent observation of Farner, Mewaldt & King (50), on the same variety of sparrow, that caged birds show a pronounced increase in nocturnal activity in spring and that this develops gradually beginning around the time that the spring molt is declining in intensity.

Wolfson (51) has noted a strong gonadal response in birds placed on a 6 hr. cycle, 5 hr. light and 1 hr. dark, similar to that seen after exposure to 20 continuous hr. of light per day. This would seem to mean that it is the daily dose of light, and not the total length of an interval, that regulates the process. This is not, however, the kind of result that had been earlier reported by Kirkpatrick & Leopold for quail; these investigators stress the importance of the dark period duration. There has been a recent discussion of this position by Hammond, Kirkpatrick & Leopold (52), with Hammond entering the disagreement and the others the rejoinder.

Descriptive accounts of mating in birds fill the late journals, but truly experimental analyses are completely lacking. There are, however, a number of excellent studies of factors in mammalian mating behavior. Grunt & Young (53), for example, have shown that a male guinea pig can be restimulated after orgasm if the first female is replaced by a second. The same authors (54) report that castration reduces guinea pigs to the same low level of sexual responsiveness, but that variations in degree of drive level noted preoperatively reappear during androgen therapy. Valenstein, Riss & Young (55) suspect that such variations have a genetic basis, but do not regard the point as settled.

A study with rats by Kaufman (56) indicates that vaginal stimulation of the penis is not essential for copulatory behavior in males, but that it is for ejaculation. The point was established by experiments with test females whose vaginas had been removed. Kagan & Beach (57) have noted that a similar effect is obtained in male rats given brief prepubertal experiences with other animals; subjects treated in this way copulate as frequently as animals reared in complete isolation, but ejaculate less frequently. This phenomenon is attributed to the learning of playful habits which compete with the mating tendency.

The care of young is an important aspect of reproductive behavior, and two new studies deal specifically with the topic. Sawin & Crary (58) have analyzed the nest-building behavior of rabbits of two closely inbred races, a subline from one of these, and a random selection of mixed animals from cross-breeding experiments. Race differences were found in time of nesting and in the quality of the nest; Sawin and Crary believe that these characteristics are determined by the balance of pituitary and ovarian hormones.

Howell & Dawson (59) have inquired into the care of young by the Anna hummingbird, a question of unusual interest for reasons reviewed by Pearson (60). The metabolic rate of this bird is so high that its time away from feeding, as at night, is typically spent in a torporous state. Nevertheless, Howell & Dawson observe that the female does not become torporous, maintains the nest temperature at a level considerably above that of the surroundings, and never abandons its poikilothermic young for periods longer than 40 min. The reasons for all this are not completely clear, but extra feeding does not appear to be a likely explanation. Conservation of heat in the resting posture and the insulation of the nest are considered to be con-

tributary, and alteration of the metabolic rate is advanced as a possibility.

It is fairly well established that reactions to young and to other animals in a social group are related to each other. New evidence for this is presented by Ramsay (61) in a study of the development of broodiness in Cochin Bantam hens. He finds that a number of distinct steps can be identified in the behavior of birds presented with chicks or ducklings for adoption, and acceptance varies with the status of the hen. Birds that are low in the pecking order adopt the chicks most readily, while socially dominant hens lag behind the others.

Boyd (62) considers family membership to be a factor determining successes in encounters between geese in large winter flocks. He concludes, in fact, that membership in a unit determines the status of a goose, and not that the status of a unit is determined by the status of a member. Success was also observed to be determined by age and by the size of the family group. Boyd identifies three main sources of conflict: sexual rivalry, restriction of movement, and threats to family coherence. The bulk of the flock consisted of *Anser a. albifrons*, but geese of other species were tolerated.

Potter & Allee (63) have shown that in hens experience with breeds other than their own alters the characteristics of contest behavior. A complete stranger of unfamiliar breed is usually challenged or attacked, but after some delay. If the hen under consideration has dominated a representative of a breed, it is likely to give authoritative displays when the next example is encountered. In other words, the hens generalize from bird to bird of a particular sort. If a prior encounter with a member of the breed was unsuccessful, the probability of exhibiting domineering behavior is reduced.

Chickens, of course, must recognize particular individuals if stable dominance orders are to form. Guhl & Ortman (64) have considered the relative importance of cues to such recognition in white Leghorn fowl. The authors conclude from their experiments that no one single feature is critical for recognition, but some are clearly more important than others. A lack of recognition was inferred from disturbances of the pecking order, and these were most readily obtained from alterations about the head. Usually, however, changes in behavior could be induced by abrupt changes in body color; slow modifications of features were generally without effect.

A problem of recognition arises in any social group, and beehives are no exception. It has been generally believed that membership in a hive is denoted by a characteristic odor and that bees without the appropriate odor are promptly stung by hive guards. The first generalization is a sound one still, but the latter requires restatement in the light of recent results. Bees are seldom hostile to strangers during the foraging season, but robber bees attempting to enter a strange hive in the fall are set upon almost at once. This attack is induced by movements, more than anything else, these being a peculiar darting behavior on the part of the robber. However, Ribbands (1) thinks that the invading bee is stung because it persists in being a nuisance and that its body odor defines it as a nuisance.

There have been a number of new experiments on the communication of bees, and all the latest disclosures are limitations upon the information conveyed by the dance. Ribbands (65) has shown that bees, alerted to a source in dishes clearly marked with color cards, go to these no more frequently than to others in the immediate vicinity. Frisch, Heran & Lindauer (66) have asked whether or not the dance indicates the height of a source, but conclude that it does not. Finally, Schick (67) has denied that bees warn a hive when they have been poisoned at a source, as had been recently proposed. All in all, however, the bee is notably "talkative" when it comes to the subject of sugar.

The bees have not been overlooked in current experiments designed to assess their relative capacity to learn the traditional problems, but Weiss (68) gives the wasp (*Vespa germanica*) a slight edge when the two are run in simple mazes with tests for reversals and for transfer. Weiss (69) states that wasps run a color-coded maze more smoothly and quickly than bees, but are more susceptible to outside interferences. Rensch & Altevogt (70), working at the other end of the size dimension, conclude that the Indian elephant is a mental as well as a physical giant. This may be the case, but the discriminative performances cited in support of the conclusion could have been obtained from almost any self-respecting rat.

A fairly accurate comparison between gorillas and other primates is now possible because of a recent study by Riesen *et al.* (71). These investigators note that gorillas solve standardized string problems in variations that are considered to be beyond the abilities of monkeys and consider this great ape to be at least the equal of the chimpanzee. Another opportunity of this sort with regard to learning-set capacities is provided by an experiment of the latter variety by Hayes, Thompson & Hayes (72), but conclusions are not as clear as they might have been because of unfortunate procedural differences. The Hayes couple (73) also report experiments with Viki dealing with her rather remarkable capacity for picture perception; and current primate information obtained in the last year is rounded out by Kuhn's (74) placing the monkey's threshold for number discrimination at a remarkable 13:12 as tested with irregular spots arranged on cards.

The work of Lepley & Rice (75), extending the concept of reactive inhibition to interpretations of paramecium behavior, has been closely followed by a similar study employing the meal worm, *Tenebrio molitor*. The findings of Grosslight & Tichnor (76) with this species are similar in most respects to the earlier conclusions: turns in an opposite direction tend to occur after a forced turn. The effect is enhanced by inserting two forced turns in the same direction and diminishes as a function of the length of a pathway between a forced and a free turn. The objection can be raised that the classical factor of centrifugal swing, as Lepley & Rice recognized for their own experiment, might well be operating in the Grosslight & Tichnor situation. Nevertheless, attempts to make interspecies applications of concepts must be given every possible welcome, for they are far too rare. Perhaps Robinson (77) is thinking

along these lines in his invocation of conditioning plus selective learning with regard to the T-maze behavior of the earthworm, but the relations between his conclusions and their analogues in mammalian learning theories are not explicitly discussed.

Only the briefest comment can be made with regard to studies of what, perhaps, is the most complex of all capacities: the ability to have a neurosis. The outstanding single publication of the year is a collection of papers under the editorship of Miner (78). The book has but one serious flaw; the old and the new are often mixed in such a way that the reader must labor to find the things that are really original. Apparently the few facts that we have are treasured above the priceless space of journals, for they tend to appear in place after place. One can also find old ideas clothed in new terms; thus Gantt's (79) principles of schizokinesis and autokinesis are reminiscent of the past struggles between emotion and intellect, Eros and Thanatos. Similarly, Liddell (80) presents a theory of anxiety and its reduction in terms that are different even though the latter are not themselves new. Mowrer (81) is clearly Mowrer, and so may Kempf (82) be Kempf, but the reviewer could not follow the latter paper.

In the realm of general discussion, the recent rise of the ethological movement is beginning to have its repercussions. One might almost be tempted to say that the honeymoon is over. As in the past, questions of theory are of paramount consideration; experimental papers remain the decided minority in the output of the school. A recent work of Spurway & Haldane (83), itself an exception to the rule, indirectly indicates the status of ethological research by its appendix on the applications of the *t*-test. Members of the school continue to place their principal faith in observation and rarely conduct the formal investigations which spawn statistical sophistication. There are a few signs, but just a few, to indicate a trend toward the latter.

Instinct, the central concept of the ethological movement, has been discussed at length both in America and abroad. Some time ago, a Cambridge round table (84) formally defined the term as

an inherited and adapted system of coordination within the nervous system as a whole, which when activated finds expression in behaviour culminating in a fixed action pattern. It is organized on a hierarchical basis, both on the afferent and efferent sides. When charged, it shows evidence of action-specific-potential and a readiness for release by an environmental releaser.

Clearly, not all of this is definition. The parts that are, with equivalents substituted for the neologisms, give a definition of instinct as "a hierarchially organized, inherited and adapted system of coordination within the nervous system which finds expression in behaviour that varies little in form, is relatively complex, and is characteristic of a species or group." This is advanced as a "more precise" usage of a term that "psychological schools of behaviour study" employ for behavior that is relatively complex, constant, and presum-

ably inherited, and related to a situation rather than a stimulus. American usage is fairly represented by the latter criteria; each is restated in the recent symposium of Allee, Nissen & Nimkoff (85). But where is the increased precision? Perhaps it is thought that making instinct a mechanism and giving it a neural reference lends respectability to the term, but maneuvers of this kind have been tried long ago. Again, it may be the stress on the hierarchical feature that is regarded as increasing the precision; if so, clarification could be a simple matter.

Active domestic interest in the concept of instinct declined for many reasons, but the problems associated with the criterion of innateness must be given the large share of the blame. These have been considered again by Hebb (86) in a beautifully clear paper; his discussion should be particularly illuminating to the ethologist who is unfamiliar with the arguments developed over the last few decades in differential psychology. The nature-nurture dichotomy is considered at much greater length by Lehrman (87) in a general critique of the Lorenz position, and his analysis should lay to rest any contention that American behaviorists have ignored the vast domain on which the ethologists have staked their claim.

The remarkably Freudian features of ethological theory have not gone unnoticed, particularly its assumptions relating to the accumulation of energy and its release through overflow in displacement activities. An application of this way of thinking is presented by Moynihan (88), who finds that removal of eggs from the nest of the black-headed gull results in an increased frequency of nest-building and preening movements. These are considered to be displacement activities which function to discharge a surplus of incubation drive. But not without protest: Kennedy (89) registers a vigorous objection to conceptualizations of this sort, citing the traditional objections to neural hydraulic models. Kennedy's antithesis is strong, to say the least, for he denounces the movement as dualistic, vitalistic, and subjective. The reactions of Hayes *et al.* (90) are far more positive. They advance a theory to integrate ethology and psychoanalysis and hope that it will be the foundation of a new science of man to be called "ethoanalysis."

Perhaps the movement will turn in response to the criticisms raised against it, but it can hardly anticipate much support in this country if it does not. Thus it seems highly improbable that American behaviorists will be permanently impressed with such analyses as those presented by Spurway (91) with regard to the relations between dogs and cats. Spurway holds that cats reared without access to the streets develop a pathological timidity; this is said to result from the accumulation of an escape drive so that release is possible through other stimuli that would ordinarily be subliminal. He suggests that dogs and cats are a drive discharge team, with cats releasing the chasing instinct of dogs. The latter, in turn, provide the stimuli releasing the fighting instinct of cats; this leads to the mutual satisfaction of otherwise starved drives. In other words, it is good for cats to be chased

periodically; the action keeps them from becoming neurotic. While spokesmen for the cats could not be reached for comment, the reviewer doubts that their reaction to the conclusion would be any less negative than his own.

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PHYSIOLOGICAL PSYCHOLOGY^{1,2}

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Physiological psychology includes all studies concerned with physiologic correlates of behavior. So wide a scope makes a reviewer painfully aware of how much he must omit. I have chosen those areas in which there seems to me most progress, or most controversy, or both. Sensory processes are dealt with in their respective chapters. The principal emphasis is on the role of the CNS³ in behavior, a choice reflecting the reviewer's preoccupation. Yet, neural (or hormonal) factors are merely specified as so many constraints which the physiology of the organism places upon its behavior. Such limited view contrasts with that of a distinguished neurophysiologist, Eccles (39), who has summarized recent oscillographic studies of the nervous system under the title, *The Neurophysiologic Basis of the Mind*.

To the reviewer the absence of any convincing physiological correlate of learning is the greatest gap in physiological psychology. Apparently, the best we can do with learning is to prevent it from occurring, by intercurrent cerebral stimulation through implanted electrodes (101, 120), by cerebral ablation (80, 117, 127), or by depriving otherwise intact organisms, early in life, of normal sensory influx (60, 92). Accordingly, we shall leave work on higher functions to the end and begin with neural and hormonal regulations of hunger, thirst, sex, and emotional behavior, then turn to the effect of electroshock convulsions, and conclude with recent work on cerebral lesions and ablations.

BIOLOGY OF "DRIVES" AND "MOTIVES": HUNGER AND THIRST

Complexity and simplicity are relative affairs. To the learning theorist, eating and drinking are simple activities leading to primary need reduction. To the physiological psychologist, phenomena of eating and drinking are problems. What tells the animal when to start, and, more importantly, when to stop with his eating or drinking? And how do animals manage to shift their preference to those substances in which they are deficient?

¹ From the Psychophysiological Laboratory, Department of Psychiatry and Neurology, New York University, College of Medicine. Preparation of this review was aided in part by the Commonwealth Fund of New York, and by the Office of the Surgeon General (Department of the Army) under Contract DA-49-007-MD-312. The review covers approximately the period from May, 1953 through May, 1954. Lack of space made it necessary to eliminate a section on electrophysiology and another on effects of drugs.

² The following abbreviations are used in this chapter: ACTH(adrenocorticotrophic hormone); CER(conditioned emotional response); cff(critical flicker frequency); CNS(central nervous system); ECS(electroconvulsive shock); PGR(psychogalvanic reflex).

Richter's demonstration (115) that rats increase their salt intake when made sodium deficient (by adrenalectomy) is generally accepted, but Richter's interpretation of the mechanism remains controversial. He assumed not only that salt depletion increases a "specific appetite" for salt but that taste sensitivity for salt is enhanced. Yet available data merely show that adrenalectomy increases a rat's motivation to discriminate saline solutions from distilled water. Pfaffman & Bare (105) made threshold determinations by recording gustatory afferent potentials (from rat's glossopharyngeal nerve) and found no difference between rats with and without adrenalectomy. To reconcile their findings with Richter's, Pfaffman & Bare suggest that there are two independent thresholds, a variable preference threshold, influenced by adrenalectomy, and an absolute physiological threshold, not influenced by adrenalectomy. How adrenalectomy lowers the preference threshold, remains unknown.

But does electrophysiology define "the absolute threshold" for salt in the rat? If Harriman & MacLeod's recent findings (55) are confirmed, the answer must be negative. They trained rats to discriminate saline solutions from distilled water, decreasing the salt concentration in small steps and shocking the rats for wrong choices. With this method they succeeded in pushing the discriminative threshold way below previously reached values. Table I [based on Harriman & MacLeod (55)] illustrates how rats have kept pace with demands made on their gustatory acuity in different laboratories.

TABLE I
DISCRIMINATIVE THRESHOLDS FOR SODIUM CHLORIDE (RATS)
Average threshold (per cent NaCl in solution) reported:

Method:	Normal Rats	Adrenalectomized Rats	Experimenters
Free choice	0.055	0.0037	Richter (115)
Free choice	0.06	0.016	Bare (8)
Recording of gustatory afferent impulses	0.008	0.01	Pfaffman & Bare (105)
Shock avoidance (large steps between trials)	0.009	0.012	Carr (29)
Shock avoidance (small steps between trials)	0.002 to 0.000025	Same as for normal animals	Harriman & MacLeod (55)

Apparently, the farther we push the discriminative threshold towards an optimal value, the less can adrenalectomy add to the rat's preference for sodium. Yet the operation does increase preference in free-choice situations where testing methods are less than optimal. How the operation does that, is just as little understood as are the factors controlling preferences in normal rats.

The normal rat adjusts his ingestion of saline, glucose, or fructose solutions to the concentration [McCleary (91)]; "pre-loading" an animal with hypertonic solutions via stomach tube or intravenous injection decreases further drinking of that solution within less than a minute. Since the taste receptors are by-passed, this control of drinking seems to depend on "post ingestion factors." McCleary suggests that the animal maintains his osmotic balance by some central control mechanism. Such "governors" may be situated in several regions of the organism, or they may be concentrated in the hypothalamus where "osmoreceptors" have been described [Verney (140)]. As Anderson (2) has shown, direct injections of hypertonic saline into these regions elicit avid drinking in well-watered goats. Euler (40) has recorded slow "osmo-potentials" from deep electrodes in these structures (supraoptic nuclei), in the cat's hypothalamus, upon intracarotid injection of hypertonic saline or glucose. More evidence is needed to decide whether these potentials are truly analogous to sensory discharges, as Euler suggests, or whether they reflect the release of neuro-secretions, since Hild & Zetler (63) have recently shown that the so-called posterior pituitary hormones may actually originate in these hypothalamic regions.

Damage to supraoptic structures may produce diabetes insipidus, excessive urination accompanied with excessive drinking. Here the drinking appears regulatory; it restores the water balance. However, section of the pituitary stalk, which does not produce diabetes, has now been shown to alter drinking behavior per se, without inducing abnormal water loss. The dogs of Barker *et al.* (9), with stalk section, "over-drunk" after periods of water depletion, as if the central governor were over-reacting to beginning dehydration. Yet the same authors caution against assumption of a single "governor"; they showed again, in normal dogs, how distending the stomach inhibits drinking, and how drinking remains self-limiting in dogs with esophageal fistulae, where the water drunk is lost before entering the stomach.

Abnormal control of eating, with hypothalamic stimulation [Hess (62)] or destruction [Miller *et al.* (94)], interacts similarly with environmental factors. Lesions of ventromedial nuclei induce overeating in rats, but as Miller *et al.* (94) have shown, the excessive eating seems more like an "inability to know when to stop." The hyperphagia is noted only on *ad libitum* feeding. The same rats eat less than controls when they have to overcome obstacles in order to start eating. Such animals also have hyperthermia, but there is no necessary association between this factor and over-eating. They are hyperthermic, even while fasting, and well before obesity sets in [Mayer & Greenberg (89)]. These observations, while unexplained, negate the beautifully simple notion that rats with hypothalamic hyperphagia eat to keep warm.

Current experiments show diencephalic regulations as important, but normally open to influences from peripheral and visceral structures. The work of Hess (61, 62) makes it abundantly clear that behavior can be influenced through direct hypothalamic stimulation with indwelling electrodes, in free moving animals. The most parsimonious explanation might be that the ani-

mal receives misinformation about his internal state and acts upon such misinformation. To the learning theorist, the diencephalic regions here discussed will probably become the testing ground for the production of "direct reinforcement" without "need reduction." To the physiological psychologist, the recent disclosures suggest that we must no longer dichotomize "central" and "sensory" mechanisms, and he marvels at the plurivalent function of such structures as the supraoptic and paraventricular nuclei which not only appear to "perceive" changes in osmotic pressure, but react to these changes with the elaboration of hormones.

THE SEARCH FOR NEURAL AND HUMORAL CORRELATES OF EMOTION

The subcortex looms equally large in current work on "emotions." Schlosberg's (123) clear statement of the "activation" theory of emotion [cf. Lindsley (83)] conceives of the intensity dimension in all emotional behavior as a continuum ranging from sleep to diffuse excitement, with the hypothalamus arousing the cortex and providing all degrees of mobilization in the autonomic systems. He thus goes beyond Lindsley who assumed that activation theory "may account for the extremes (of emotion) but leaves intermediate and mixed states unexplained." Schlosberg even proposes to measure the intensity dimension; he is quite hopeful that general level of skin conductance (not PGR!) might be the correlate.

The differentiation of more specific emotions from "diffuse excitement" may be an ontogenetic process [Hebb (60)]. One of Hebb's students, Melzack (92), reared dogs in restricted environments (narrow boxes) and confronted them several weeks after release with various innocuous but "emotion-arousing" objects (toy balloons, live rabbits, etc.). Normal dogs of the same age showed avoidance, but diffuse excitement prevailed in the restricted dogs. When the tests were repeated 10 to 12 months after release, the restricted dogs showed as much avoidance as the controls; but they also displayed diffuse excitement, while the free environment dogs showed aggression. These studies suggest that differential rearing might profitably be added to the design of ablation and stimulation experiments on emotion, but most students of the subcortex seem to have implicit faith in "instincts" and take their organisms as they find them.

The major concern of those who experiment on the neurology of emotion seems to be how to take the Klüver-Bucy syndrome apart. Klüver & Bucy (78), it will be recalled, first described these striking behavioral changes in monkeys after bilateral temporal lobectomy. The changes include perceptual difficulties bordering on "psychic blindness," over-reactivity to visual stimuli, strong oral tendencies (mouthing of objects, sniffing), altered dietary habits, hypersexuality, and marked emotional changes (increased tameness, lack of reactions generally associated with anger and fear). The lesions resulting in this syndrome involved the temporal neocortex (lateral and ventral) as well as rhinencephalic structures (viz. amygdaloid complex and hippocampus). This syndrome has now been taken apart by making more selec-

tive lesions; temporal lobe lesions in lateral and, especially, ventral neocortex can produce the "visual" difficulties without any obvious emotional change [Blum *et al.* (18); Mishkin & Pribram (99); Mishkin (97); Riopelle *et al.* (117)], and emotional alterations can be produced without apparent perceptual deficit, by restricting the lesions to the rhinencephalon [Pribram & Kruger (111); Walker *et al.* (142); Weiskrantz (146)]. One may rightly ask whether these "parts" of the syndrome, taken together, actually add up to the complex Klüver-Bucy picture; they probably do not [cf. Walker *et al.* (142)], but for the time being, we shall discuss the emotional changes, and leave other aspects of the syndrome aside (see below).

Simultaneously with the Klüver-Bucy study, Papez (103) enunciated his theory of emotion, proposing as anatomical substrate the "circuit": hippocampus→fornix→mammillary body→anterior thalamic nuclei→cingular gyrus→cingulum→hippocampus; normal emotional experience as well as expression are said to depend on the integrity of this circuit. Klüver himself (77) feels that his evidence is not incompatible with Papez' concept, and MacLean (86) has elaborated it by terming all structures included in Papez' circuit, as well as septal, orbito-frontal, insular, and temporo-polar regions, collectively, the "visceral brain." The grouping of all these structures into a supposed functional unit is based on comparative anatomical evidence [MacLean (86)] and on strychnine studies purporting to demonstrate their interrelations [MacLean & Pribram (88); Pribram & MacLean (112)]. However, the concept of a "visceral brain" is called into question by Kaada's survey (73); most of the structures included in this "visceral brain" respond with somatomotor, as well as visceral effects to electrical stimulation. The same point is stressed by Pribram & Kruger (111) in their impressive review of anatomical, physiological, and psychological studies of the "olfactory brain." There is rather general agreement that the greater portion of the rhinencephalon has other than olfactory functions, but these functions need not be visceral and may be quite heterogeneous.

What are the roles of individual sections in Papez' circuit, and related structures? Recent work has centered about the amygdaloid complex, the anterior limbic region (cingular gyrus), and various subcortical structures (viz. septal, thalamic, and hypothalamic nuclei). We shall take up the evidence in this order. The amygdala has been an area of predilection both for ablation and stimulation experiments, partly because of the controversy surrounding the effects of lesions in this structure. Bard & Mountcastle (7) reported that extensive decortication in cats failed to produce sham rage as long as the amygdala was spared and that amygdala lesions, by themselves, produced savageness. These results were puzzling in view of Klüver's work and subsequent studies which indicated, for the monkey, that amygdala lesions produce "placidity" [Klüver & Bucy (78); Walker *et al.* (142); Weiskrantz (146)]. The only possible conclusion, at that stage, was to propose that the cat is simply not a monkey.

It now turns out that amygdectomy in cats may also lead to placidity

and, apparently, predominantly so. Schreiner & Kling (124) recorded persistent placidity in 18 of 20 amygdalectomized cats; the remaining two showed savageness, as described by Bard. No differences in operative lesion were found that might account for the divergent behavioral results. In addition, all but four of Schreiner & Kling's animals became markedly hypersexual after the operation; the four that did not nevertheless were placid, indicating that even these features of the "syndrome" (viz. placidity and hypersexuality) have no predictable relation to each other. In the monkey, too, placidity may follow amygdalectomy without hypersexuality since only placidity was observed by Walker *et al.* (142) and the workers in Pribram's laboratory [Weiskrantz (146); Mishkin (97)].³ But how can we account for the inconsistent results (placidity and savageness) in the amygdalectomized cat?

There are two obvious possibilities: first, the differences may be attributable to differential handling of the animals, pre- or postoperatively; or, second, to variations in the nature of the lesions (irritative versus destructive). On the first notion, effects of amygdalectomy might be relatively nonspecific [Weiskrantz (146)], perhaps similar to a series of ECS;² or to some depressant drugs such as sodium pentobarbital which, as Lindsley & Jetter (85) have shown (for dogs), can temporarily eliminate a learned discrimination as well as "fear," in the sense of a learned avoidance response. The operation would then not produce a specific taming effect, but merely render the animal abnormally susceptible to the influence of postoperative treatment, be it friendly or otherwise. Behavior after amygdalectomy has not been analyzed sufficiently to confirm or refute this theory of nonspecific change. According to Weiskrantz (146), amygdalectomized monkeys fail to acquire conditioned avoidance responses (shuttle box) to the experimenter as unconditioned stimulus; yet, they are nearly as adept as normal monkeys in learning to avoid electric shock. Amygdalectomized cats, however, at least when under the care of Brady *et al.* (22) take much longer than normal cats in acquiring shock avoidance. Differences between experimenters, species experimented upon, and methods may all have contributed to these divergent results.

There is considerably more agreement on effects of stimulation of amygdala and adjacent structures. Stimulating the phylogenetically old (anteromedial) portion of the amygdaloid complex through implanted electrodes in waking animals (cat), Kaada *et al.* (74) produced sniffing, licking, clawing, and various autonomic effects. Stimulating the phylogenetically new (basolateral) portion yielded "searching" (to contralateral side), apparent "bewilderment," and at times "expressions of fear, anger, and fury." Quite similar results are reported by Gastaut *et al.* (47), and, particularly, by MacLean & Delgado (87) who record and illustrate "organized angry" and "aggres-

³ It should be noted that the studies cited employed sexually immature animals, in contrast to Klüver & Bucy (78).

sive" responses in cats and monkeys with electrodes implanted in amygdala and adjacent rhinencephalic structures (pyriform cortex and hippocampus near the amygdala). The same results were obtained on application of acetylcholine to these areas [MacLean & Delgado (87)]. All these authors (47, 74, 87) stress the striking similarity with temporal-lobe epilepsy in man [cf. Penfield & Jasper (104)].⁴ The predominance of fear may reflect an unfamiliar or disorganizing influence of the stimulation which sets up diffuse after-discharges. The similarity between Bard's earlier results of ablation and current reports on stimulation makes it rather likely that the earlier results of amygdectomy were attributable, in part, to stimulating (irritative) effects of the procedure, just as some stimulation experiments suggest that their effects are temporary paralyses.

We can only skim over some of the other way-stations on Papez' circuit. For hippocampus and fornix, our current evidence is rather negative. Bilateral lesions of the hippocampus apparently do not duplicate the results of amygdectomy [Walker *et al.* (142); Mishkin (97)], and bilateral section of the fornix, according to Pribram & Kruger (111) has no discernible effect on behavior. The role of the cingular gyrus in emotional behavior has likewise been questioned. To Papez the cingular gyrus is as specific a receiving area for emotional experience (under influx of impulses from hypothalamus and hippocampus) as is the striate cortex a primary cortical receiving system for vision [Papez (103)]. Such specificity is hard to accept by anyone who believes that emotional experiences are related to widely distributed patterns of central nervous activity [Lindsley (83)]. To be sure, stimulation of anterior cingulate ("area 24") had been claimed to produce powerful "suppressor" effects on ongoing electrocortical and motor activity, and cingulectomy [Ward (144)] has been quaintly described as leading to "loss of social conscience" in the experimental monkey. Neither of these claims can be upheld. The specific "suppressor" function of this region is presently under the same clouds that have descended upon other supposed suppressor "bands" of the mammalian cortex [cf. Sloan & Kaada (130)], and the supposedly dramatic results of cingulate ablation have been subjected to an "experimental critique" by Pribram & Fulton (110). Their cingulectomized monkeys exhibited normal reproductive activities and no change of hierarchy status in social colonies when each animal's pre- and postoperative rank was compared. Needless to say, cingulectomy as a variant of psychosurgery in man continues to be performed with solemn, if uncritical, reference to the earlier animal literature (139). In the rat, bilateral lesions of "median" cortex are reported by Stamm (132) to reduce hoarding activity, roughly in proportion to the amount of "median" cortex destroyed. It is curious that

⁴ Penfield & Jasper (104) pointed out that spontaneous "fear" in temporal lobe seizures might well be part of the attack and not simply fear that an attack is about to come on; still, in their experience, neither localized epileptic discharge nor electrical stimulation ever led their patients to report "anger, joy, pleasure, sexual excitement," although "fear" was frequent (104).

Smith *et al.* (131) found a similar reduction of hoarding activity in hamsters with intact brains, but adrenalectomized. In work on anterior limbic structures, just as in that on amygdaloid complex, there is an unfulfilled need for biochemical studies coupled with systematic observation of behavior.

Travelling on Papez' circuit from old cortex and transitional cortex (cingulate) to subcortical structures, we reach the "septal region" of the forebrain. Confirming earlier reports of savageness in rats with septal lesions, Brady & Nauta (21) describe dramatic changes in emotional reactivity (fierce fighting, explosive startle reactions), particularly after supracommissural septal destruction. Almost as striking as the immediate postoperative changes (in direction of savageness) is the rapidity with which these changes disappear (12 days). The effects are perhaps similar to the results of the drastic experiments reported by the Tulane group [Heath (57)] who implanted electrodes into "septal" forebrain regions in schizophrenics and terminal cancer cases. They recorded what they considered abnormal electrical activity from the schizophrenics, and then stimulated the septal region in the hope of "disrupting" this abnormal discharge. Recourse to such procedures reflects disappointment with therapeutic effects of psychosurgery as usually practiced.

Still, we would prefer to see more work on the role of subcortical nuclei in experimental animals before hastening to clinical application. Compared with what is known on effects of cortical ablations, the information on subcortical structures is quite sparse. Schreiner *et al.* (125) describe contrasting behavioral effects of stereotaxic lesions in different thalamic nuclei, lesions of the mediodorsal nucleus (cat) leading to "heightened irritability," and of the anterior nuclei, to abnormal "docility." Results of combined lesions in both nuclear masses are described as "labile behavior" with over-reaction to both noxious and pleasurable stimuli. Clearly, we need sharper categories than docility or savageness, but the value of contrasting subcortical lesions with each other and with ablations in corresponding cortical projection zones is obvious. We also need more studies of combined and seriatim lesions in different nuclear masses. An example is the observation by Schreiner & Kling (124) in one of their male cats, rendered "placid" and hypersexual by amygdalectomy. This animal subsequently received a hypothalamic lesion (ventromedial nuclei) which abolished the placidity, and produced a viciously aggressive animal [see also Wheatley (147)]. Yet, much too little is known regarding the nature of these behavioral changes, and the physiological mechanisms which might intervene between the placement of the diencephalic lesions and the appearance of grossly pathologic behavior.

Recent work by Porter (107, 108), McCann (90), and others (37, 64) is of considerable significance in this connection. Porter (107) observed that the release of ACTH² by the pituitary in response to "stress" was regularly preceded by characteristic alterations in the electrical activity of the posterior hypothalamus. These changes, recorded through subcortical electrodes in cats and monkeys, began within half a minute after the injection

of epinephrine (adrenalin), or slightly later if the "stress" was insulin or hypoxia, and lasted up to 15 min. Destruction of this region abolishes ACTH release to stress. Direct stimulation of the region in the otherwise intact animal produces the traditional signs of ACTH release, namely, drop in circulating eosinophiles and lymphocytes [De Groot & Harris (37); Porter (108)], and disappearance of ascorbic acid from the adrenal [McCann (90)]. To show that the changes in electrical activity are necessary antecedents of the pituitary secretion, Porter (108) injected ACTH directly into his animals while recording their electro-subcorticograms. There was prompt reduction of the electrical activity, indicating that the hypothalamic activity is not a result of circulating ACTH, but vice versa, and furthermore, that the hypothalamo-pituitary system is normally self-calibrating with the hypothalamus triggering the ACTH discharge, and ACTH in turn damping the hypothalamic activity. These disclosures may provide one of the bridges from neural to humoral events—bridges required by any "activation" theory of emotion. But does this make the hypothalamus into the organ of emotion and motivation, or, to go as far as Stellar in his recent review (133), a congeries of organs, governing various emotional and motive states? Even Hess (62), who has spent many decades on studies of the diencephalon, warns us not to impute to these structures any exclusive role, in isolation from cortex and lower brain stem. Research in recent years has placed an amazing burden of functions on the hypothalamus, a structure weighing 4 to 5 gm. in the adult human, whose total brain weight is about 1360 gm. For how long will the hypothalamus be able to carry its burden?

ELECTROCONVULSIVE SHOCK

Administration of ECS to rats seems to have lost none of its popularity among experimenters, although it ought to be somewhat less popular among rats. The reason for continued enthusiasm may be that convulsive behavior is easily observed, and that there is always hope of finding clues, from rat studies, for a better understanding of ECS effects in man. The two major questions in this area are: (a) how does ECS exert its effects on behavior? and (b) what is the nature of these effects?

Progress has been made toward an answer to the second question in the work of Hunt, Brady and their various collaborators. These studies utilize a conditioned emotional response (CER) "of the 'anxiety' or 'fear' type" [Hunt & Brady (65)]. The rat is trained to press a lever for an aperiodic water reward. A clicking noise is introduced and terminated after 3 min. by a painful shock to the rat's feet. After six such trials the animals, on hearing the clicker, stop pressing the lever (or press much less), "freeze," "crouch," and defecate. A series of 21 ECS (three a day for one week) abolishes this CER² for over 30 days [Hunt, Jernberg & Brady (66)]. However, after 30 odd days, the fear response returns, unless extinction trials are interspersed (66) or additional ECS are administered [Brady, Stebbins & Hunt (24)]. Thus, whatever ECS does, it seems to do it reversibly and its effects

are the more lasting the sooner one starts the ECS treatment after the CER has been established. How the CER manages to become consolidated with time is as obscure as are the reasons for the transient effects of ECS. There is furthermore the paradox that ECS-treated rats are more emotional, and quite literally need to be handled with gloves, yet the major experimental effect is the diminution in the conditioned emotional response. In this respect, however, the effects of ECS in rats are similar to those of septal lesions [Brady & Nauta (21)] which produce a transient rise in irritability and a concomitant depression in a CER acquired before operation. A reversible change in some humoral factor is our best guess to account for effects of ECS or of septal lesions, but it is a mere guess, in the absence of adequate biochemical evidence.

Besides knowing a little more on what ECS does to the rat, we now have some information on what it cannot do. Stone (134) shows that it does not tame feral rats, even though in domesticated rats, such "instinctive" patterns as maternal behavior and nest building may be seriously disrupted [Rosvold (119)]. There is further evidence that, in domesticated rats, ECS may affect drive states, but the situation is complex. Mirsky & Rosvold (96) have shown that food intake in rats was reduced for as long as 10 days after a series of ECS, if the animals were kept on *ad libitum* feeding throughout the experimental period. But in rats kept at 85 per cent body weight, during and after ECS, there appears a significant increase in food intake (as soon as they are given free access to food), and there is a corresponding increase in hunger "drive," if measured by bar-pressing for a food reward. ECS thus leads rats that had been on restricted diet to over-correct a mild deficit, just as pituitary-stalk section did in the study of Barker *et al* (9) whose dogs overdrank after water deprivation. In view of their findings, Mirsky & Rosvold (96) rightly warn us against generalizing from studies of ECS, in which effects of ECS on learning are assessed under conditions of food reward. Contradictory reports on effects of ECS, in the current literature, may be attributable to differences in feeding schedule which might have affected motivation. The problem is the same as in ablation studies where deficits in performance may at times be a result of changes in motivation to perform, rather than in the animal's ability to solve a given task. In fact the concept of an ability independent of motivation may be as elusive as Kant's "thing by itself" which is as unattainable as it is indispensable to Kant's system.

As to the mode of action of ECS, we can be somewhat more definite. Apparently, the convulsion seems to be more important than the electricity, since there is complementary evidence to show that current without convulsion may be ineffective, while convulsions produced without current may have similar effects as does ECS. If ECS is given to rats under ether anesthesia (so that the convulsion is prevented), the usual deficit in maze-running fails to appear [Porter & Stone (106)], and there is no disruption of gestation and maternal behavior [Rosvold (119)]. However, sodium dilantin, in con-

trast to ether, does not protect the rat against deficit in maze-running [Thomas & Stone (138), confirmed and extended by Broadhurst, Stone & Lawrence (27)]. It may be significant that ether prevents the seizure altogether, while sodium dilantin merely modifies it; rats given ECS under dilantin have abortive fits, "running fits," and a few show tonic-clonic convulsions. Possibly, only complete prevention of seizures, as under ether anesthesia, is adequate to dissociate the effects of passage of current from those of the convulsion. Further evidence is given in a series of experiments by Hunt, Brady and their co-workers [Hunt, Jernberg & Lawlor (67); Hunt, Jernberg & Otis (68); Brady, Stebbins & Galambos (23)]. Using the CER as index, Hunt, Jernberg & Lawlor (67) showed that with ECS given under ether anesthesia, the CER is not abolished, although just as much current passes through the rat's head as without ether anesthesia. Conversely, when convulsions are induced without current, viz., chemically [by carbon disulphide, *vide* Hunt, Jernberg & Otis (68), or through the jangling of keys (audiogenic fits, Brady, Stebbins & Galambos (23)], then the CER is depressed, indicating that the convulsions (or their antecedents or sequelae in the organism) are the crucial factor, and not the passage of current per se.

The convulsion, in these experiments, was sufficient, but was it necessary, or could some other treatment induce identical behavioral effects? From the work reviewed in the preceding section, we might surmise that some limited intracerebral stimulation may have effects equivalent to those of ECS applied through scalp and skull. A much simpler method is advocated by Hayes (56); he studied maze learning in rats subjected to temporary anoxia (produced by manual compression of the rat's chest). The effects of such "treatment" were indistinguishable from those of a series of ECS. Yet much more behavioral evidence is needed to prove that the effects of ECS and temporary anoxia are equivalent. To establish such a point is difficult as long as we do not know the crucial physiologic aspects of ECS in rats. Nor do we really know these for man, despite Gellhorn's impressive survey of possible mechanisms [Gellhorn, (48)]. Yet, our understanding of ECS is still better than that of psychosurgery and of the effects in subhuman primates of frontal ablation which led to the introduction of frontal lobotomy and its many variants.

EFFECTS OF CEREBRAL LESIONS (NEOCORTEX) IN ANIMALS AND MAN

During the period under review, effects of cerebral lesions have been studied with a vigor which recalls the heroic age of our field, three quarters of a century ago. Increasing numbers of these studies concern old cortex (rhinencephalon) and subcortical nuclei, and have been reviewed above. But the neocortex remains the principal area for localization of function. This classical problem is actually three problems in one: We begin with the discovery of symptoms of lesions; we try to assign these symptoms to specific lesions in given areas; we attempt to analyze the altered performance which underlies the symptom (49). All too many studies stop short at discovery and localization of symptoms, i.e., with the question of "where," and fail

to proceed to the analysis of altered function, i.e., with the question of "what" to localize. These considerations apply with equal force to studies of primary projection systems and to those of so-called "association" cortex. As we shall see, symptoms of lesions in primary projection systems may not be as simple and specific, nor those of so-called "association" areas as diffuse and complex, as used to be believed.

Even anatomically, the facile dichotomy of sensory and association cortex is difficult to maintain. As Chow & Hutt note in their recent review on the "association" cortex of *Macaca mulatta* (32), it is impossible to define "association areas" except by exclusion, viz. all neocortex, outside of sensory and motor projection areas and cingular gyrus. Yet, the discovery of second and even third sensory areas (104, 118, 148) and of possible "polysensory" regions (93) by evoked-potential methods [cf. also (114)] continually encroaches on "association" cortex.⁵

In studying functions of primary sensory systems, the best we can do, under these conditions, is to start from some unequivocal sign of sensory deficit following cortical lesion, and to analyze the nature of the deficit as completely as possible. In this sense, analysis of function has logical priority over analysis of structure.

Nature of "primary" visual deficits.—Bay's important work on visual deficits in brain-injured men is now available in an English summary [Bay (14)]. In it he describes the methods he developed with Cibis and Lauenstein for measuring local adaptation ("fading times") in the visual field for eccentrically placed targets. This fading is enhanced in cases of visual field defect, even in those parts of the field which seem intact on routine perimetry. Thus, a circumscribed defect (scotoma) is accompanied by subtle but systematic changes elsewhere in the visual field. As Bay points out, the same conclusion has been reached by others, using a rather different method, viz. flicker-perimetry which reveals reduced cff^2 in the seemingly intact regions of visual fields exhibiting localized homonymous field defects [Teuber & Bender (136)]. Such observations suggest that visual function, in the presence of focal lesions of central visual pathways, may be altered in a complex manner which is not adequately described by the usual method of plotting scotomata and giving acuity data. Since these latter methods are usually the only ones applied in cases of visual agnosia, one can raise serious ques-

⁵ Such difficulties of cortical parcellation have general significance. There are at least five methods of subdividing the cortex: by major lobes, cytoarchitecture, thalamic projection, evoked potential (and electrical stimulation), and, finally, by strychnine neuronography. Each of these methods has its ambiguities, though some have more than their share [e.g., cytoarchitecture, cf. Lashley & Clark's critique (82) and Bailey & Bonin (6)], and especially strychnine studies [Frankenhaeuser (43); Wall & Horwitz (143)]. But the main point is that each of these five methods yields a different map. To harmonize such differences can only detract from the major task of finding out (by ablation experiments) whether any, and if so, which of these structural subdivisions have functional significance.

tions regarding the nature of this syndrome [Bay (12, 13); Semmes (127)]. Is agnosia really a dissociation of "higher" and "lower" visual functions, with disruption of higher (cognitive) aspects of vision, in the presence of essentially normal primary function? This reviewer agrees with Bay that none of the cases of visual agnosia thus far reported is compelling on this point. Subtle changes in primary visual function (e.g., abnormally rapid fading) may produce a semblance of agnosia without requiring the assumption that higher aspects of vision were selectively impaired.

Changes in somesthesia.—The same might be said for somesthesia, where deficits after cerebral lesions are the more complex, the more refined the methods for their discovery. Thus, testing both hands of a brain injured patient, either simultaneously, or in rapid succession, brings out tactile deficits not revealed by single stimulation of one or the other hand alone. A recent study by Weinstein (145) is a case in point. Men with penetrating brain wounds were classified into two contrasting groups: those with diminished position sense and light-touch threshold (for von Frey hairs) constituted a "sensory" group; those without these symptoms constituted a brain injured but "nonsensory" group. All patients estimated weights placed by relays on their supported palms, either simultaneously on both palms [bilateral simultaneous method; cf. Bender (15)] or in rapid succession. These methods revealed marked constant errors of relative judgment between the hands; in the brain-injured sensory group, weights on the affected hand were significantly overestimated; in the brain-injured nonsensory group, a similar error appeared, but of opposite sign, i.e., weights on the hand contralateral to the lesion were underestimated. There thus was a subtle involvement of tactile function, even in those who seemed normal on traditional (unilateral) tests of sensation. Despite their subtle nature, the deficits were persistent; most of the patients had sustained their injuries 8 to 10 years prior to testing.

In subhuman primates with neocortical lesions, somatosensory functions are considered somewhat more resilient. Cole & Glees (33) removed a small portion ("hand area") of postcentral gyrus in monkeys and assessed effects by quantitative tests of motor power, finger dexterity, and tactile discrimination (grab-bag technique of Ruch). Immediately after operations they found some drop in motor power (more marked in contralateral hand), considerable diminution in finger dexterity, and complete loss of "stereognosis." On the grab-bag test, the animal fumbled among the objects to be chosen, closed its hand, brought it out empty, and "looked surprised." After the initial drop, performance on all tasks showed considerable recovery, ascribed by Cole & Glees (33) to multiple overlap of projection in the primary sensorimotor cortex, rather than to functions of the "second" sensory area. Yet, neither Cole & Glees, nor anyone else has been able to apply to monkeys as intricate tests as those described above for men.

A direct attempt to clarify the relative importance of first and second sensory area has been made by Zubek for the rodent (149). Rats were peripherally blinded and trained on a tactile "form" discrimination problem (cylin-

der versus wedge). This discrimination was not affected (a) by bilateral cortical removals anterior and posterior to somatic areas I and II [as defined on Woolsey's maps (148)], nor (b) by bilateral removal of somatic area I and II, either separately or together.

The picture one obtains from these three studies in somesthesia (33, 145, 149) is that of increasing resiliency of somatosensory function as one descends phylogeny. Yet correlated with this descent is, of necessity, an increasing coarsening of behavioral methods, so that the resiliency on lower levels might be more apparent than real.

Gustation.—A special aspect of this difficulty is the uncertainty, in all tests of sensory acuity, regarding the control of motivation (see above, page 276). Are the dietary changes observed as part of the Klüver-Bucy syndrome the result of altered taste thresholds, or are they merely a reflection of the more general loss of avoidance behavior? In two studies, Bagshaw & Pribram (5, 109) clarify this point by showing that bitemporal lobectomy may change dietary habits in monkeys from herbivorous to omnivorous, without affecting the acceptance threshold for quinine, and, conversely, ablations in the neighborhood of the anterior insula do alter quinine acceptance thresholds (transiently), although no change in dietary habits can be observed (5, 109). Such double dissociation of symptoms argues against interpreting the dietary changes as the result of gustatory deficits. Yet, it does not mean that the changes in dietary preference are therefore of a higher "associative" order, i.e., a gustatory agnosia in the absence of primary taste deficit. We need only recall that dietary choices depend on a great deal besides taste and that bitemporal lobectomy has strong effects on the animal's motive state.

In summary, it should be clear, at least for vision and somesthesia, that lesions of primary projection systems do more than produce simple and circumscribed losses of sensory function; they also lead to complex alterations in mode of sensory functioning, alterations which for the most part have yet to be described. Evidence in other sense modalities is even more fragmentary but it is still possible that alterations from lesions in any projection system transcend the primary sensory processes, supporting Lashley's notion that receptive cortical areas have more than receptive function (81). Conceivably, any neocortical lesion large enough to produce symptoms has a double effect: a specific one, depending on locus, and a general one, perhaps depending on size.

Studies of anterior and posterior lesions (outside of primary projection systems).—Harlow (52) and Chow & Hutt (32) have recently surveyed effects of lesions outside of primary projection areas in macaque. Contrasting prefrontal with parieto-temporo-preoccipital areas, we find tasks which become selectively difficult with bilateral anterior lesions (in the absence of demonstrable sensory deficit), and other tasks which are rendered selectively difficult by bilateral posterior lesions (again, without encroachment on known sensory projections (18, 53). The anterior syndrome involves failure on

delayed-response and delayed-alternation tasks (usually, but not necessarily, combined with hyperactivity); such failure is rare and transient after comparable posterior ablations. The posterior syndrome involves rather persistent failure on relatively simple tests of visual discrimination learning (18, 30, 31), but little or no deficit on delayed response (53). In other words, effects of anterior and posterior lesions show double dissociation of symptoms, indicating some specificity of function (52, 53). On the other hand, there are certain complex tasks (notably so-called conditional reactions) on which macaques with anterior or posterior lesions do equally badly as compared with normal animals [Lashley (79); Chow & Hutt (32)]. Such nondifferential failure speaks for generality in the effects of lesions of so-called associative cortex. Whether to stress the specific or nonspecific changes is, at this stage, largely a matter of personal preference. No one has pushed the behavioral analysis of individual deficits far enough to assign the proper weights to either kind of results.

Anterior lesions.—To produce the symptom of the anterior region (delayed response deficit), symmetric ablations from the dorsolateral aspect of the frontal lobes are sufficient [Blum (20) for macaque; Pribram *et al.* (113) for baboon]. Lesions in the ventromedial aspects of the frontal lobes fail to engender the symptom. However, while lateral frontal removals of cortex are sufficient, they may not be necessary. Rosvold & Delgado (120) have demonstrated that electrical stimulation or destruction (electrocoagulation) of small areas in the head of the caudate nucleus produced a picture in the monkey indistinguishable from the much more extensive removals of frontal cortex. In their work multilead electrodes were implanted, by a variation of Hess' classical method (61, 62) into the anterior lobe substance of monkeys trained to perform on delayed response and visual discrimination tests. Stimulation in the caudate, during testing, made delayed response performance fall to chance level, as long as stimulation lasted, while visual discriminations continued undisturbed. Destruction of these points led to loss of ability for delayed response and to persistent hyperactivity. The study is remarkable in showing that stimulation and destruction need not be opposites. It suggests further that some symptoms routinely attributed to frontal cortex may be referred more properly to the anterior caudate, a point taken by Muskens 20 years ago (102). Chemical lesions of caudate are implicated in the forced-turning syndrome by Aprison, Nathan & Himwich (3); disturbance of some postural component of behavior, or of interaction between vision and posture, may be more important in generating the delayed-response deficit than any supposed disturbance of higher functions.

For it is true that we simply do not know what dimensions of behavior are measured by our delayed response tests. By varying the stimulus situation, or the internal state of the animal, the task of delayed response may be made either soluble or insoluble to the bifrontal macaque, but so far we cannot rationalize our data. Sedating the bifrontal animal may improve delayed response in some cases [Mishkin *et al.* (100)], partly confirming Wade (141)

and contradicting Blum *et al.* (17). Changing the testing situation in various ways, but always so that the animal's cue before the delay becomes "non-spatial," may make the delayed-response test soluble to the monkey even without the benefit of prefrontal cortex [Mishkin & Pribram (98)]. The bifrontal animal will learn to go to the right cup (in the traditional two-cup situation) five sec. after a reward has been shown, and to the left cup 5 sec. after an empty hand has been shown. (This is an instance of nonspatial delayed response). Yet, the same animal is totally unable to perform the classical "spatial" delayed response test, i.e., to go to the correct cup after 5 sec. delay, depending on whether the right or the left cup had been baited. What goes awry after the frontal operations is certainly not memory *per se*; but is it memory for place? The problem does not become any simpler by noting that delayed-response tests may lose their specific difficulty for frontal preparations as one moves from the lower primates to the anthropoid apes. Reports on two chimpanzees at the Yerkes Laboratories (with extensive bifrontal lobectomies sustained at two months of age) stress normal performance on delayed response [Blum (19)] as well as on complex Weigl-type sorting tests [Evarts & Nissen (42)]. These negative results are, of course, in striking contrast to the earlier ones obtained by Jacobsen *et al.* (69) in the two famous chimpanzees, "Lucy" and "Becky," at Yale. Blum (19) and Evarts & Nissen (42) further noted that reaction to frustration was much more violent in their two operates than in control animals, again in contrast to the earlier notions on effects of frontal lobectomy [Jacobsen *et al.* (69)], which led to the development of psychosurgery in man. The only sign of frontal lobectomy in the Yerkes Laboratory chimpanzees was an **inconstant hypermotility**.

In lower forms, hypermotility has often been reported after frontal lesions; in the macaque the symptom is as reliable as is delayed response deficit, but it is actually even less understood, since we know neither the location of the minimal lesion (latero-dorsal? orbital? caudate?) nor the nature of the behavioral alteration. In rats, Zubek & De Lorenzo (150) attempted to show that lesions of both frontal poles of the brain are sufficient to produce hypermotility, while selective ablation of dorsal frontal or of lateral frontal cortex is ineffective. Yet, inadvertent involvement of anterior caudate structures (by interference with blood vessels) remains a possible factor in the production of hypermotility [as for other symptoms traditionally ascribed to prefrontal cortex, see Muskens (102)]. Some degeneration of caudate is found after any prefrontal lesion [Harman *et al.* (54)].

Posterior lesions.—Symptoms of posterior "association cortex" are discussed in the articles by Harlow (52) and by Chow & Hutt (32), and most recently, for temporal lobe, in the superb review by Milner (95). The locus of the minimal sufficient lesion for visual discrimination deficit is as yet undefined, but it is apparent that nearly complete ablation of prestriate cortex (areas 18 and 19, so-called) does not produce this symptom [Lashley (79); Evarts (41)], although a few studies without anatomical verification continue to implicate these "visuo-psychic" regions [Riopelle & Ades (116)].

On the other hand, bilateral temporal lesions appear to be sufficient to produce the deficit (30, 97, 99). These observations thus confirm Klüver's & Bucy's reports (78) on visual disturbances after bitemporal lobectomy, but such radical ablation is not necessary; removal of cortex from lateral [Chow (30, 31)] or ventral temporal cortex [Mishkin & Pribram (99)] may be sufficient. The data of Mishkin & Pribram (99) and of Mishkin (97) indicate that ventral temporal neocortex might be more important; neither invasion of lateral neocortex nor of hippocampus or amygdaloid region was necessary in their experiments.

If the problem of locus of this temporal deficit is not quite solved, the more basic question of the nature of the deficit has barely been attacked. Mishkin (97) and Mishkin & Pribram (99) were able to show that severity of postoperative deficit varies with the preoperative difficulty of the task, a color discrimination being least affected by the operation, and a pattern discrimination most. Yet, what is it that is disturbed in these animals? To say that visual discrimination is impaired, is grossly inadequate as a description of the nature of the deficit. With the exception of Klüver's & Bucy's animals (78), which had radical temporal lobectomies, none of the recent reports based on selective ablations have recorded disturbances severe enough to be noted in the animal's cage behavior. The impairment is brought out only on formal testing, and even there some visual tasks fail to reveal the impairment while others do. Thus Riopelle *et al.* (117) were able to show that performance on a series of patterned string tests was unimpaired by temporal removals, although the order of difficulty of different subtests was altered. On the other hand, series of discrimination tests (for dissimilar objects) revealed a deficit in that the operated animals failed to show improvement from problem to problem, although they were able to learn each individual discrimination. Riopelle and co-workers suggest, therefore, that the temporal lesions had interfered not with primary learning but with the acquisition of learning sets, i.e., the ability to learn how to learn, as defined by Harlow (51). Riopelle's study represents the most sophisticated approach, so far, to the analysis of temporal deficit; nevertheless many problems remain.

Lashley (81) has raised the question whether we have sufficient proof that the deficit is specifically visual and not rather a sign of more general impairment of comparison behavior, or perhaps, of comprehension of the training situation. To demonstrate specificity of the deficit for visual discrimination we need to do more than show that discrimination in some other modality, e.g., somesthesia, is unimpaired. Such simple dissociation might indicate merely that visual discrimination is more vulnerable to temporal lesions than tactile discrimination. This would be a case of hierarchy of function rather than separate localization. What is needed for conclusive proof is "double dissociation," i.e., evidence that tactile discrimination can be disturbed by some other lesion without loss on visual tasks and to a degree comparable in severity to the supposedly visual deficit after temporal lesions. Conclusive evidence of this sort is still lacking, although Blum, Chow & Pribram (18)

and Semmes (126) have shown that temporal ablations, by themselves, do not impair performance on tactile latchbox and form discrimination problems in the monkey. The same authors demonstrated, however, that deficits in these tests, after parietal lesions, were enhanced once they extended the lesions from the parietal region into the posterior cortex of the temporal lobes. To show conclusively that the temporal lobe deficit is strictly "visual," we would also need data on both auditory pattern discrimination and on vision for the same temporal lobe preparation; but such auditory data are not available. The methods, however, are outlined in the work of Diamond & Neff (38) who trained cats to discriminate tonal patterns which contained the same frequencies but in different sequential arrangements. After bilateral combined ablations of first, second, and third auditory areas [AI, AII, and Ep of Rose & Woolsey (118)] the discrimination was lost and could not be relearned. Yet, discriminations based on a simple change in frequency could be learned. It is quite conceivable that the visual discrimination deficit after temporal lesions is likewise a function of how the question is put, experimentally, to the monkey. The complexity of so-called simple discrimination learning for sub-human primates is underscored in an ingenious study by Jarvik (72). He showed that a hue discrimination can be taught to experimentally naïve animals (chimpanzees, rhesus and spider monkeys) in one to two trials, if colored bread was used instead of the customary colored plaques. Intermediate techniques, e.g., covering bread with colored celluloid or pasting it on colored plaques, gave intermediate results. Jarvik's study, as well as Diamond & Neff's (38) and Riopelle's (117), suggest methods which ought to be applied in attempts at elucidating the peculiar temporal lobe syndrome.

Comparison of lesions in anterior and posterior "association" cortex of man.—Can observation of brain-injured man yield contrasting symptoms of anterior and posterior lesions, in analogy to those found for subhuman primates? The predominant view has been that the human "association" cortex, in contradistinction to specific sensory and motor systems, mediates higher (intellectual) functions and that the anterior (frontal) areas are considerably more important in this respect than posterior (parieto-temporo-occipital) regions. Evidence for the pre-eminent role of the anterior region comes from study of tumor cases [Rylander (121, 122); Halstead (50)] indicating that surgical removal of neoplasm from the frontal lobes leads to greater psychological deficit than similar removals from other portions of the cerebrum. The generality of these findings, however, has been questioned [Harlow (52); Hebb (59)],⁶ and contrary evidence has been given for cases

⁶ The change in emphasis is reflected in the new version of Klebanoff's review on psychological effects of cerebral lesions and ablations [Klebanoff *et al.* (76)]. This new version is organized around etiologies and symptoms and not around major cerebral lobes, as was the earlier version [Klebanoff (75)], which tended to dismiss intellectual deficit in cases of involvement outside the frontal lobes by suggesting that there might be in most cases some unknown encroachment on frontal structures.

of penetrating missile wounds of the brain, where performance on a variety of complex visual tasks was either equally impaired after anterior or posterior lesions, or slightly more impaired with the latter [Teuber (135)]. Similarly, Birkmayer (16), reviewing his World War II experience with "over 1,000 cases" of penetrating battle injuries of the brain, declares that on all tests men with frontal lesions did somewhat better than those with injuries in other lobes.

The simplest explanation for the apparent discrepancy between studies of tumor cases and those of penetrating brain wounds was to attribute the difference in results to the different etiology: tumors in the frontal lobes might be more disruptive of cerebral functions than the passage of a missile. This simple explanation, however, has been rendered difficult by the report of Battersby *et al.* (10). Patients with pre- or postrolandic tumors were tested on slight modifications of tasks used earlier for patients with missile wounds, and the results compared with a control group consisting of cases of spinal cord tumor or increased intracranial pressure of unknown origin. Patients with anterior or posterior tumors did worse than the controls, but anterior and posterior cases did not differ from each other. The study was based on consecutive admissions of patients, and any patient at all testable was tested. Rylander (122), however, explicitly states that he omitted from his group all patients with "aphasia, agnosia, or apraxia." Since the most severe and persistent forms of such syndromes occur with posterior lesions, the selection process alone would account for the finding of seemingly greater impairment in the frontal group.

The present evidence indicates that, on certain tasks, frontal as well as postcentral lesions can produce significant deficits in man. But the evidence does not mean that one could not identify specific symptoms of anterior or posterior lesions if the tasks were appropriately constructed for such purpose. No one has reported deficits in men with frontal lesions which would approach the specificity of delayed-response failure or hypermotility in macaque.⁷ Reasoning that delayed response might involve a complex interaction of vision and posture, Teuber & Mishkin (137) asked their patients to set a luminous line, in the dark, to the vertical, while the patient's body was tilted. As Aubert (4) has shown long ago, normal observers err under these conditions, but men with frontal brain wounds were now found to make significantly larger errors than men with posterior lesions or normal controls. Conversely, men with posterior lesions made larger errors on setting a thread to the vertical against an interfering background while their body was upright. Such double dissociation of symptoms indicates specificity, even in the absence of detailed localization of anterior and posterior lesions.

⁷ Following injury to the frontal lobes in man, various forms of "disinhibition" may occur, e.g., motor restlessness, garrulousness, sex talk or indecent behavior [Jarvie (71)]. Yet the incidence of such changes in any large series of cases with frontal involvement is relatively rare, nor is it clear whether similar changes might not at times appear with cerebral lesions in other than frontal areas.

For lesions of parietal lobes of man symptoms of considerable specificity have traditionally been reported (e.g., spatial disorientation), although their relation to primary sensory involvement is not clear. In fact the severity of changes thus far recorded for animals with large parietal ablations [Semmes (126)] comes nowhere near equaling the impressive, if bewildering array of clinical syndromes, so magnificently reviewed in Critchley's recent book (36). These syndromes in man are usually based on descriptions of acute cases and are notoriously variable; but even 10 years after parietal injury subtle symptoms are demonstrable by suitable methods. Thus, Semmes *et al.* (129) have subjected the symptom of spatial disorientation to an experimental analysis. Men with frontal, parietal, temporal, and occipital brain injury, and controls with peripheral nerve wounds were required to follow routes of graded complexity by means of maps. The group with parietal lesions was significantly inferior to the controls and to all other brain-injured combined; the brain-injured without parietal lesions did not differ from the controls. The difficulty in route-finding, however, was not a form of "visual agnosia" (as often asserted in the clinical literature), since it appeared regardless of whether the maps were presented through the visual or tactual modalities.

Men with parietal lesions also differed, as a group, from other brain-injured by failure to show transfer from a series of complex visual to analogous tactual tasks involving sorting, matching-from-sample, and conditional reaction [Semmes *et al.* (128)]. To paraphrase Harlow's terminology, they failed to acquire "intersensory learning sets." The results suggest further application of tests in series requiring transfer of information from one sense modality to another.

It should be noted that neither these authors (128), nor Critchley (36) believe that the parietal lobe, or any of the cerebral lobes, constitutes a functional unit; grouping of subjects by lobes is a matter of convenience, until more appropriate subdivisions become possible. The extent to which posterior parietal symptoms might be distinct from signs of temporal lobe involvement, is particularly doubtful, although Milner (95) has shown that lesions of either temporal lobe in man may be followed by impaired performance on certain "visual" tasks requiring the arranging of pictures to tell a story or the discovery of absurdities in outline drawings. Difficulties with "visuo-constructive" tasks (block design) may follow right temporal lesions, but the defect again is not modality-specific, since it can be demonstrated equally on using a tactual formboard [Hebb (58); Milner (95)]. These deficits are remarkable in that they are apparently independent of the presence of aphasia.⁸

⁸ Experimental study of the aphasias has been completely neglected during the period under review, even though methods for analyzing expressive (1) and receptive (70) disorders are available. The relatively language-free tasks described above (128, 129, 137) are suited to the investigation of the nonverbal aspects of aphasic disturbance.

Stimulation of the exposed temporal cortex of man, as reported by Penfield & Jasper (104), leads to visual or auditory hallucinations or both, to complex "dreams," and to "memories," i.e., subjectively convincing recurrence of earlier experience. These observations may eventually help in our understanding of effects of temporal lesions, though few of us would accept Penfield's interpretation of the temporal lobes as "memory cortex."

Nor would we like to subordinate the cortex, as Penfield & Jasper suggest (104), to the upper brainstem and diencephalon (as a "centrencephalic system" which controls cortical activity). It is true that bilateral lesions of basal brain and diencephalon disorganize cortical activity more effectively than any restricted neocortical lesion [Conrad (34, 35)]. We may need the diffuse activating systems to stay awake (28, 44, 84): but the cortex, in turn, can activate the brainstem [Bremer & Terzuolo (25, 26; see also 11)]. Work on reticular formation therefore need not lead to the assumption that the cortex is a lesser organ. But the continuing disclosures on "diffuse projection systems" carrying impulses in addition to those in classical, specific afferent paths (45, 46) may help in understanding the effects of cortical lesions. Future work will decide whether the duality of specific and nonspecific systems can explain the twofold effects, specific and general, of neocortical ablation.

POSTSCRIPT

If progress in a field consists of advances in methods, the gathering of new facts, and the clarification of concepts, then the year under review has been blessed on the first two counts. Increasing concern with the role of subcortical structures in behavior has led to further adaptation of Hess' implanted electrode technique and to a greater awareness of interaction in the CNS between neural and humoral regulations. Ablations of neocortex and rhinencephalic structures have become more selective and are more often reported with adequate histological controls. Work on animals has profited from assimilation of testing techniques originally devised for man (42, 53) and work on men with cerebral lesions has been equally aided by the application of tasks used earlier for nonverbalizing organisms (128, 135). We are seeing the beginning of a truly comparative approach to effects of cerebral lesions or stimulation. So far, results in man and subhuman primates are in better agreement than we dared to expect. What is still lacking is a rigorous appraisal of physiological events intervening between placement of a lesion (or stimulation) and appearance of altered behavior. Implanted electrode studies and work on effects of ECS will benefit if stimulation is combined with recording [Monnier & Laue (101)] and if chemical changes during and after stimulation are assessed [Porter (107, 108)]. Similar developments are desirable in work with adrenalectomy.

Yet no degree of refinement of ablation or stimulation technique can substitute for clarity of concepts referring to structure and function. We still find uncritical reference to one or another mode of parcellation of cor-

tex or subcortex, as if the parcellations themselves ("primary areas," "association cortex," "visceral brain") yielded functional systems. Our greatest hazard, however, lies in the use of inadequate terms in the description of altered behavior. What are "amnesia," "tameness," "savageness," "visual discrimination deficit," except mere symptoms which await further experimental analysis? Unless we work on our concepts, the accumulation of facts will hinder rather than help the advance towards a synthesis of neurology and psychology (81).

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ABNORMALITIES OF BEHAVIOR¹

(IN THE LIGHT OF PSYCHOGENETIC STUDIES)

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In accordance with the broad editorial policies validated by tradition, the selective trend of this review of recent advances in psychiatric and psychological genetics will be toward an integrated rather than a complete report on the pertinent literature of the past two years. Consistent with this scheme, relevant publications on human behavior problems will be considered from a genetic viewpoint on the clinical side, and from the standpoint of their potential psychological significance on the genetic side. Specialized reviews covering some sections of the defined area can be found in the *Archives of Internal Medicine* (22), the *American Journal of Psychiatry* (70, 72), the *Acta Geneticae Medicae et Gemellologiae* (92), and the *Regensburger Jahrbuch für ärztliche Fortbildung* (122). A continuous record of bibliographical references of genetic interest is maintained by Phelps in the *American Journal of Human Genetics*.

BOOKS AND SYMPOSIA

The steady progress of psychiatric genetics toward an austere solidified status of scientific respectability was documented by the appearance of numerous specialized books suited for purposes of instruction. Their volume varied from the pocketbook edition of *Genetics* by Kalmus (74) to Sorsby's oversized textbook of *Clinical Genetics* (132), the co-operative product of 31 British, American, Scandinavian, and Swiss writers. Chapters of particular psychological interest were contributed by Böök (oligophrenia), Dahlberg (biometrics), Herndon (cardiovascular disorders), Klein (metabolic disorders), Neel (detection of carriers), Pratt (neurological disorders), Slater (psychiatric disorders), and Waterhouse (twin studies). The list of the remaining contributors was of comparable selectivity and comprised many well-known names. Apart from an excellent introductory chapter, Sorsby's personal contribution was limited to a section dealing with ophthalmological aspects of clinical genetics.

The textbooks of Kemp (80) and Lamy (85) were also oriented toward problems of clinical genetics and included sections on neuropsychiatry, but understandably on a limited scale. Even more general in contents were the books published by Darlington (24), Goldschmidt (42), Hovanitz (55), and Srb & Owen (134), while those by Haldane (47) and Williams (141) dealt

¹ The period covered by this review extends approximately from May, 1952 to April, 1954.

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largely with problems of chemical genetics. The titles of the first four publications listed were *The Facts of Life*, *Understanding Heredity*, *Textbook of Genetics*, and *General Genetics*. The books by the last two authors were published under the titles *The Biochemistry of Genetics* and *Free and Unequal*, respectively.

Two new texts focused entirely on psychiatric genetics were entitled *Heredity in Health and Mental Disorder* (69) and *The Biology of Mental Health and Disease* (139). The former grew out of the nineteenth series of the Thomas W. Salmon Lectures and was presented by one of the reviewers. The latter incorporated the contributions made by 92 investigators to the twenty-seventh conference of the Milbank Memorial Fund and was edited anonymously by Kruse. The exceedingly broad scope of this conference was compared by Gerard to the apparent confusion observed in the building of a termite nest. Nevertheless, he predicted that some day all the tunnels and galleries built by the termites would connect and that the many pillars signifying "understanding of cellular organization, brain function, and mental behavior" would join perfectly. The formidable difficulties encountered in an attempt to comprehend in a purely abstract manner the synthesis of the myriad elements, which constitute the life cycle of a human organism, were also stressed by Glass (40) and Kallmann (69).

Various specialized symposia in the field of psychiatric genetics testified to the growing realization of the need for coordinated interdisciplinary approaches to an understanding of complex behavior problems. The sessions were organized around such diversified topics as the genetics of constitutional defects which may associate themselves with certain types of intellectual subnormalcy or early atherosclerosis (98), the components of intelligence (102), methodological problems in human genetics (49), genetic counseling (26), and the usefulness of genetic training for psychiatric marriage counselors (114). Several sessions at the Ninth International Congress of Genetics in Bellagio (109) and at the First International Symposium of Medical Genetics in Rome (108) were assigned to similar subjects.

A two-day conference of the Association for Research in Nervous and Mental Disease was devoted to a comprehensive discussion of the genetic basis of integrated neurological and psychiatric patterns (54). At the symposium, the program of which had been prepared by Hooker, Whitehorn and Snyder, topics of special psychological interest were presented by Anastasi (inherited and acquired components of behavior), Carmichael (phylogenetics of behavior patterns), Davis (enzyme formation control), Gesell (fetal-infant behavior), Glass (adaptability), Jervis (phenylketonuria), Kallmann (psychotic behavior patterns), Kanner (constitutional inadequacies in early infantile autism), and Thompson (intelligence). At the Italian conferences, problems of psychological genetics were dealt with especially by Gedda (aims of medical genetics), Kallmann (genetics of mental health and mental disorder), Penrose (recent trends in human genetics), and Sjögren (progress in psychiatric and neurological genetics). Penrose emphasized the urgency of

combining the search for evidence of allelic series or closely linked sets of genes in quantitative traits with the study of conditions of genic equilibrium in continuous traits like intelligence, while Kallmann stressed the need of extending the groundwork of psychodynamic theory to the dynamics of gene-controlled phenomena.

The Rome conference was held at the new Gregor Mendel Institute, the inauguration of which was highlighted by a policy-setting address of Pope Pius XII in support of ideologically unshackled research in human genetics (1). The reference to modern studies of human heredity as "perhaps the most dynamic research" of our time promised to be helpful not only in dispelling the demoralizing threat of the current brand of Lysenkoism, that is, the theory that scientific thinking should conform to political thinking, but also in refuting the false although still widespread notion of a basic conflict existing between religious tenets and the scientific principles of human genetics as applied to medicine in general and to the management of behavior problems in particular.

GENERAL METHODOLOGICAL PROBLEMS

The difficulties encountered in attempts to apply genetics to psychiatry "in order to enable the clinician to distinguish syndromes in a less subjective and disputable way than is possible at present" were forcibly described by Slater (130). He deplored the division of psychological medicine into schools, some of which reject even the most fundamental standards of classification accepted by others, often with such an emotionally toned attitude as to remind one of the days of debate between "allopaths" and "homoeopaths" over a century ago. Proceeding from violently contested ground, the worker in psychiatric and psychological genetics finds his position weakened, if only because he does not believe that all forms of mental disorder are genetically equivalent. He is compelled, therefore, to adopt some system of classification. Despite these man-made difficulties in "helping to provide the needed precision" in a causative mode of approach, Slater felt certain that genetics would be able to supply the specific cause of a number of mental disorders.

That genetics has not even been able as a basic science to enjoy ideological freedom from intradisciplinary disunity was the eloquently substantiated topic of Goldschmidt's presidential address at the Bellagio congress (43). The two divergent philosophies were identified by him as the statistical (static) and the physiological (dynamic) points of view. The statistical philosophy was described as hyperatomism and hyperselectionism and was criticized for attempting to interpret every generalized set of facts by the introduction of more and more units for statistical treatment. This pattern was assumed to lead eventually to impossible consequences by requiring astronomical numbers of modifiers and a similar number of tiny but specific adaptations, merely because it tried to explain all basic features of genetic phenomena by introducing more genes in the form of modifier systems built up by selection. In the dynamic approach, preferred by Goldschmidt al-

though it accepts the basically statistical tenets of genetics, the main objective was seen in an understanding of general phenomena in terms of genic action and developmental systems with all their consequences of interaction, embryonic regulation, and integration, rather than in "... looking for explanations in terms of unproved, additional systems of units for more and more genic permutations."

The statistical problems to be mastered in the complex analysis of the genetics of quantitative variation were expertly outlined by Nedler (97), Spuhler (133), and Wright (143). Spuhler explained why "we must use inefficient methods of analysis" if we are to investigate continuous variation in man. He did not believe that we will soon know enough to have a predictive understanding of the genetic system underlying quantitative variability, assumed to represent the combined effect of multiple gene loci (distributed over the chromosomes) and environmental determiners. According to Wright, who is given credit for developing the theoretical aspects of the interaction of the four factors which can upset existing gene frequency equilibria and establish new gene frequencies (mutation, selection, mixture between breeding populations, and genetic drift), statistical analysis is relatively simple, if the hypothesis of additive action of genes both within and between loci is assumed. However, if pleiotropy and interaction are involved, a process of trial and error among multiple more or less independent lines was shown by Wright to be required. A method of attacking the problem of genetic drift, which may influence gene frequencies in small human isolates, was successfully employed by Glass *et al.* (41) in the study of an Old Order "Dunker" (Old German Baptist Brethren) community in Pennsylvania.

The statistical biases, which may be introduced into genetic data on discontinuous characteristics in man, were discussed by an impressive panel of experts at the Bethesda conference (49), notably by Schull (121), Macklin (90), and Dempster (25). There was general agreement that the problem of adequate ascertainment of probands and controls was not easy to solve in selecting families for the study of any one or some combination of four basic questions: namely, to determine (a) the mode of inheritance of a specific character, or if this is known, (b) its linkage relationships with other characteristics, (c) the selective advantages or disadvantages of the character bearer, and (d) the frequency with which the trait arises spontaneously, presumably as a result of mutation.

The problems posed by the use of the multiple variance method in determining "the nature-nurture variance ratio" with respect to functionally but not necessarily genetically unitary traits were neatly specified by Cattell (19). In a still incomplete study of 12 measurable personality factors, comparable test data were in the process of being obtained from five different populations, in pairs: namely, (a) a sample population of identical twins in their own families, (b) siblings in their own families, (c) siblings with each member of the pair in a different family, (d) unrelated persons in pairs in the same families, and (e) unrelated persons in different families.

PROCEDURAL PROBLEMS IN TWIN STUDIES

The growing awareness of the excellent opportunities offered by the twin study method not only resulted in a revitalization of interest in human genetics, but also brought to the field a great number of controversial issues and methodological difficulties, peculiar to disciplines concerned with human behavior problems [Kallmann (71)]. Therefore, twin researchers were cautioned against the tempting tendency to draw generalized conclusions from incidental observations in single pairs of twins and against being misled into describing temporary and perhaps easily reversible dissimilarities between the adjustive responses of twin partners in the antithetic setting of dichotomies. With respect to the majority of human traits, Kallmann questioned the rationale of aiming at the assignment of exact quantitative values to the relative contributions of genetic and environmental factors in the production of individual differences. On the same basis he doubted the validity of arguments minimizing the representativeness of twin data because of some readily appraised differences between twins and single-born persons. Observations which seemed to call for a certain degree of caution in this respect included the studies of Bauer (7), Burlingham (18), and Husén (56).

Bauer studied one pair of one-egg twins and, notwithstanding pronounced similarities in their interests and behavior patterns, noted considerable differences in their personality organization. For instance, the twins were found to attach different meanings to common activities, although their external behavior was described as almost identical. One twin seemed to be social-minded, but was classified as passive in terms of personality structure. The other twin, although outwardly displaying a tendency to withdraw, sought more meaningful relations in the environment than his co-twin. They impressed the investigator as being psychologically incomplete individuals, who "shared an ego" and projected each other's inner life in their manifest behavior.

Burlingham's report was limited to the early histories of three twin pairs, chosen from an institutional series of seven sets and presented as monozygotic without specification of the procedure which was used in classifying them. The histories were obtained in the war years 1940 to 1945 and covered the first three years of life in two pairs and a three-year period beginning at the age of 3½ years in the third pair. Two pairs were fatherless children, while the third pair was described as unmanageable, retarded in speech, and awkward in movements. Obviously, it would seem rather difficult to disregard the very specific institutional conditions under which these three pairs were observed, and even more difficult to accept them as representative of "the educational problem presented by twins" or of "the unparalleled opportunity provided by them for distinguishing constitutional from acquired characteristics."

The main purpose of Burlingham's study was to analyze "the environmental and innate conditions which account for the differences between the development of twins and that of ordinary children." Although only a small

segment of the complex problem of sibling rivalry was taken into consideration and no allowance was made for differences in family backgrounds, basic personalities, and adjustive patterns, expected to be as diversified in twin subjects as in single-born persons, twins were found to have "a more acute rivalry to cope with than is the case with ordinary siblings." Compared with the rivalry among siblings, that between twins was thought to start at an earlier age, to be more pronounced "because" of the necessity of competing on an equal footing, and to culminate more frequently in early mutual death wishes. Other potentially disadvantageous aspects of twin development were seen in a weakened relationship to the parents as a corollary of an intensified identification process between the twins themselves and in a bewildered parental attitude toward two children who cannot be told apart without difficulty. The mechanism of identification was assumed to preserve similarity between twin partners, which otherwise would gradually yield to significant differences in behavior.

Burlingham's interpretive approach also appeared to be open to criticism because of the tendency to think in terms of behavioral opposites, even when the differences observed were temporary and reversible. For instance, the early differentiation of roles, said to divide pairs of identical twins into one active and one passive partner, was found to be "... determined by the bodily strength of the children and to change according to changes in their relative health and development."

Husén's study (56) dealt with the intelligence test scores and public school records of twins and single-born students. Like other investigators whose school-age twin data will be reviewed in another section, he reported that twins scored significantly lower on intelligence tests and included a significantly higher proportion of cases showing subnormal intellect than was true for single-borns. With respect to scholastic subject achievement, he presented evidence for an enhanced similarity of school performance ratings in one-egg twins, and claimed, rather questionably, to say the least, that high similarity in subject achievement might be attributable to such environmental influences as going to and from school together and being mistaken for each other by teachers. In his sample, such mistakes on the part of the teaching staff were held to have occurred in 89 per cent of the one-egg twins.

In a study which compared the IQ's of preschool and school-age twins and single-born controls, Zazzo (144) observed an over-all IQ reduction of 9 units in the twin population and a mean difference of 5 IQ units between the scores of same-sexed and opposite-sexed pairs of twins in favor of the latter group. He ascribed a part of the overall difference to biological factors (increased prematurity rate in twins, lower weight at birth, higher maternal age) and the remainder to a language retardation associated with a purported social isolation in twins. However, the deleterious effect in the language sphere, attributed to the twins' preferential use of a "secret language," appeared to have been overstressed.

That multiple births are distinguished by an excessive incidence of prematurity (Karn (76)) and birth trauma was confirmed by Allen (3) in an institutional series of mentally retarded twins. In the group of defectives diagnosed as posttraumatic, the proportion of twins ascertained showed a statistically significant increase from the expected twin rate (2 per cent) to 5.3 per cent, although the given category included mental deficiency cases attributable to postnatal trauma. It also was observed that a considerable proportion of cerebral-palsied twins were survivors of adverse circumstances which had been fatal to their twin partners.

No data were supplied by any investigator, which might have revealed an indication that premature babies are more likely than full term babies to develop a severe psychosis, or that mental illness in general might be considerably more prevalent in twins than in the general population. Therefore, Kallmann stressed on several occasions (54, 69, 71) that once a twin has come through the ordeal of being born, he is biologically undistinguished from single-born individuals, even to the extent of having a complete ego of his own. The main limitations of twin studies were seen by him in the following circumstances: (a) twins cannot be separated before they are born, nor can they be provided with two mothers of different age, personality, or health status; (b) two-egg twins are no more dissimilar genotypically than brothers and sisters and like them, are rarely raised in different cultures; therefore, even fraternal twins are unlikely to fall into the extremes of theoretically possible genetic and cultural differences; and (c) the average difference between one-egg twin partners is no precise measure of environmentally produced variations, nor does an increase over the average difference between two-egg twins represent the exact contribution of genetic influences even in relatively comparable environments.

The advantages of the twin-study method were summarized as follows: (a) it constitutes an excellent sampling procedure for the study of variations displayed by different genotypes in a controlled environment or by a constant genotype under the influence of different environmental conditions; (b) in the investigation of traits requiring close personal contact with the research subject, it provides an innocuous approach to families who might not otherwise be willing to give information about their private lives; (c) in conjunction with the statistical principles of the census, proband and sibling methods, twin studies can be used as an economical substitute for a total population survey, or as an effective procedure combining the advantages of family studies with the application of test procedures requiring experimentally controlled conditions (twin-family method).

Regarding the use of the modern similarity method, it was confirmed by various investigators that dermatoglyphics and blood factors could be regarded as the most reliable criteria for distinguishing one-egg and same-sexed two-egg twins, especially if fingerprints are analyzed quantitatively as well as qualitatively. The quantifying procedures used for this purpose were much improved by Slater (129), who employed Fischer's method of discriminant functions.

SPECIAL PERSONALITY TRAITS

The psychopathological significance of constitutionally determined personality types was emphasized by Langfeldt (86). The most common types were described by him as the sensitive, cyclothymic, schizoid, hysterical, compulsive, paranoic, and paranoid. The "sensitive constitution" was assumed to play an important part in a variety of psychoneurotic and psychotic reaction syndromes.

Interesting data on the complex background factors of outstanding intellectual or artistically creative ability were reported by Juda (65) and Roe (117). Juda's study received its impetus from Lombroso's *Genius and Mental Illness*, wherein mental disorder had been considered an essential concomitant of genius. It was based on the personal, marital, and genealogical histories of 294 highly gifted persons, who had been born within the German language area between 1650 and 1900 and had made notable contributions in the arts and sciences. The sample of artists (113 in number) consisted of sculptors, painters, poets, architects, and composers. That of the scientists (181 in all) included high-ranking representatives of the biological, physical, social, and technological sciences as well as theologists, philosophers, educators, and statesmen. Of their more than 18,000 relatives (ancestors, siblings, descendants), approximately 5,000 were available for personal interview. The investigation of a control group of 115 artists and scientists of lesser achievement remained incomplete.

The ancient theory of mental disorder being a prerequisite for outstanding ability was clearly refuted by Juda's data. By and large, the index families were found to have been distinguished from those of average people by a predominance of achievement rather than by an increased tendency to mental illness. Compared with the scientists (4 per cent cyclical; no case of schizophrenia), the artists had a higher rate of schizophrenia (2.7 per cent). They also had a lower fertility rate, a shorter life span, increased celibacy and divorce rates, a higher number of illegitimate offspring, and an increased infant mortality among their children. Only the families of the scientists had a high enough reproductive rate to insure their survival. Low intelligence was rare in the descendants of both artists and scientists. The index cases themselves were more frequently observed to have been first-born children than would have been expected by chance.

With respect to the interaction of genetic and nongenetic factors in the development of outstanding intellectual or artistic ability, Juda's findings confirmed the complexity of the formative processes involved. In general, it appeared that a preparatory ground in the form of certain ancestral talents was required in combination with healthy character traits. Except for the poets whose relatives showed diverse scientific and artistic abilities, the same type of talent displayed by artists (including composers) tended to recur in their families. In the group of scientists, only the relatives of mathematicians revealed a consistent trend toward similar ability. Otherwise, various com-

binations of talents were as conspicuous in the background of scientists, as manual dexterity was among the ancestors of certain groups of artists, or rhetorical ability among those of poets. In addition, the ancestral consanguinity rate was found to be high.

Some of Juda's carefully formulated conclusions were corroborated by the results of Roe's study, which dealt with the personal, educational and adjustive histories of 64 prominent American born scientists and a control group of university faculty members of lesser distinction. With respect to the elite group in the natural, physical, and social sciences, the patterns distinguishing life histories, intellectual abilities, and personality types were found to differ from those of average people and to be relatively specific within certain scientific categories. Like their German counterparts, many American scientists were first-born children and came from selected families. Over one-half of their fathers had been professional men, as against a general average expectancy of 3 per cent. A considerable proportion of the parents revealed evidence of unusual ability, while the essence of the family derived stimuli was seen in the value placed on knowledge and learning, for its own sake, rather than for economic or social rewards. That genetic and environmental influences interact in producing a highly gifted person was also concluded by Roe.

Irrespective of the rather popular notion that exceptional intellectual ability in women is disproportionately rare, Roe's study was limited to males, presumably for purposes of homogeneity. There were two women in Juda's sample which had been ascertained with the co-operation of many academic agencies, one painter and one poetess. To those sophisticates, however, who might want to infer that the genetic potentiality for superlative achievement in the arts and sciences seems largely confined to men, Montagu (96) offered a challenging refutation. Fortunately (from the standpoint of the reviewers) Juda and Roe represented the female sex, but Montagu did not.

GENETIC PROBLEMS OF INTELLIGENCE

The previously mentioned observation of a deficit of approximately five IQ points in the mean intelligence score of twin subjects (Zazzo) received some support from the data which accrued in a longitudinal psychometric study of British school children [Lorimer (89)]. The only demonstrable factor capable of accounting for some of this difference between twins and non-twins was seen in the disproportionate frequency of multiple births observed in women in their late thirties. Children, who are born fairly late in the maternal period of reproduction, are likely to come from relatively large families, and there is a negative correlation of the magnitude of $-.19$ to $-.26$ between sibship size and intelligence test scores. Lorimer conceded that the exact cause of the IQ deficit in twins was not readily apparent at this time, but he suggested that adverse conditions of gestation and birth might frequently result in relatively inconspicuous impairments of cerebral structure and intellectual function.

According to Feingold (31) who tested 240 English-speaking, noninstitutionalized twin subjects over age 60 (mean age 69.7), there was no evidence of a difference between the test performances of senescent twins (identical and fraternal) and comparable single-born persons. This was consistent with Zazzo's finding that the difference between the intelligence quotients of twins and nontwins tended to decrease with increasing chronological age. The battery in Feingold's study included tests of reasoning ability, vocabulary knowledge, immediate recall of digits, visual and meaningful verbal material, formation of new associations, and speed of tapping. The analysis revealed significantly smaller mean intra-pair differences in the scores of one-egg twins than in those of two-egg twins, but only on tests measuring abstract intellectual abilities and in female pairs. The test data in male pairs showed a similar trend, which was not statistically significant on any of the tests. The observed sex difference appeared to indicate that certain intellectual changes in the period of senescence are observable in males at an earlier age than in females.

Generally corroboratory psychometric data on adolescent twins were reported by Thurstone *et al.* (138). Their sample consisted of 48 one-egg and 55 two-egg pairs, who were given an extensive battery of tests measuring chiefly primary mental abilities, personality, and psychomotor function. The preliminary analysis appeared to raise almost as many questions as it answered, as a result of inconsistencies in motor function scores for the two hands and the failure of reasoning and numerical abilities to distinguish the two zygosity groups. Nevertheless, the study substantiated the assumption of an important genetic component in those abilities differentiating one-egg and two-egg pairs of twins. Especially on some of the visual, verbal, and motor tests, two-egg twins displayed marked intra-pair differences with significantly increased frequency. Equally decisive results were reported by Keiter (78, 79).

That considerable difficulties are encountered in the genetic analysis of human intelligence "known only through intelligence tests" was ascribed by Thompson (54) to the fact that these tests are generally constructed on an arbitrary basis and lack structural referents. He also expressed the belief that the highly complex manner, in which heredity and environment contribute to a test score, may vary with the stage of development and thus preclude a clear separation of their individual contributions, especially in man. A similar view was placed on record by Anastasi (54) with the statement that "heredity and environment are themselves abstractions, each referring to a multiplicity of specific factors which interact with each other." On this basis, at least, Herndon (48) seemed justified in likening attempts to evaluate genetic factors in human intelligence to "trying to catch smoke in a basket."

That his state of resignation was more apparent than real was evidenced by his interesting data on the distribution of Wechsler-Bellevue scores among 223 persons, who belonged to 86 Watauga County families (Blue Ridge

Mountains). In the investigator's opinion, this population provided an example of a normal distribution of IQ scores in a group with an unusually high rate of cousin marriages and presumably with a small size of mating isolates. The observed intrafamily correlations (mean IQ score, 94.52 ± 0.87) were regarded as "... consistent with usually accepted estimates that the genetic contribution to the total measured variation of normal intelligence is not less than 50 per cent nor more than 75 per cent."

The rather optimistic attitude reflected in a recent statement of a special UNESCO committee [Wolfe (142)] regarding the widely observed negative association between intelligence and the number of children per family seemed to be somewhat at variance with the inferences drawn by Osborn (101) and other experts in population genetics. According to the former group, there is at the present time no reason for "great concern over an impending decline in intelligence." The effects of differential ability were thought to be small at best, although the effects of differential fertility may have acted to keep the average from improving; but "... whatever the effect, it seems to be disappearing." In Osborn's opinion, which was based on data collected in a large-scale survey in Indianapolis in 1941, there was a definite need for measures which would increase a sense of economic security in well-endowed individuals and reduce the pressures favoring late marriage. In view of the direct relation between planned family size and economic status, and in line with the current trend toward ever-increasing use of fertility control by contraception, he predicted that "to a far greater extent than in the past, the genetic basis for man's higher qualities of intelligence and personality will, for good or evil, be sorted out for survival by individual choice as to births."

PSYCHONEUROTIC BEHAVIOR PATTERNS

The psychopathologically important but vaguely defined area of ill health, commonly referred to as psychoneurotic in American terminology, continued to be beclouded by a scarcity of comprehensive genetic data, possibly because it involves continuously variable vulnerabilities to stress symptom formation calling for the statistical techniques of analysis of variance. According to Eysenck (29), many differently formulated theories of neurosis are on the same descriptive level and are concerned with the same fundamental dimension of personality. In collaboration with Prell (30), he was one of the few investigators of psychoneurotic reaction potentials (outside the field of criminality) to avail himself of the opportunities afforded by the twin-study method. On the basis of their study of 25 one-egg and 25 two-egg pairs, Eysenck & Prell classified "the neurotic personality factor" as a biological and largely gene-specific entity, estimating the genetic contribution to this "neurotic unit predisposition" at 80 per cent.

Slater's twin observations (129, 130), although numerically limited (9 one-egg, 43 two-egg pairs), were interpreted by him in the sense that psycho-

neurotic symptoms are exaggerations of polygenically determined personality variants and less closely related to a given type of stress than to the basic personality. Despite "almost identical personality," only 2 of the 9 one-egg pairs were found to be concordant as to psychoneurotic symptoms, as against 15 pairs in the two-egg series. The histories revealed that critical deviations in a man's career might be caused by relatively chance occurrences, such as the personality of the chosen partner in marriage. It was found that one twin "might suffer a mischance which would lead to a vicious circle of ill-health, social failure, hardship, discouragement and increased ill-health, while the other totally escaped." In line with his theory of graded constitutional vulnerabilities along more than one dimension, Slater assumed that "the man who breaks down with a neurotic illness is likely to be handicapped not by one constitutional weakness of severe degree, but with a number of minor weaknesses."

In a study of 62 same-sexed pairs of twins between the ages of 12 and 15 years (36 one-egg, 26 two-egg), ascertained through the schools of four adjacent South London areas, Shields (127) found one-egg twins (69 per cent) to be twice as likely as two-egg twins (31 per cent) to have the same degree of adjustive difficulty, when rating each child "on a four-point scale of psychiatric maladjustment." Compared with single-born controls, twins of either zygosity revealed no evidence of a higher incidence of neurotic adjustment problems, but male twins and nontwins far exceeded their female counterparts in presenting some difficulty in adjustment. Little more than one-half of the total group of school children investigated were classified as nonneurotic. Nongenetic explanations for the observed differences between one-egg and two-egg twins were rejected especially with respect to concordance as to the type and severity of neurotic behavior patterns.

As regards overt homosexual behavior in the adult male, Kallmann (67) observed a nearly perfect concordance rate in 44 one-egg pairs, with the index cases standing at least midway on the homosexuality scale applied and with pronounced similarity in the role taken by twin partners in their individual sex activities. In the two-egg group (51 pairs), nearly 60 per cent of the co-twins of predominantly or exclusively homosexual index cases showed no evidence of overt homosexual behavior at any age, and only 11.5 per cent were given homosexuality ratings of five or six on Kinsey's scheme. In this series, all concordant twin partners denied any mutuality in overt sex relations, but Dalla Volta & Zecca (23) reported twin incest in a pair of opposite sex. The likeliest genetic hypothesis accounting for the development of at least some forms of adult homosexuality was seen by Kallmann in a gene-controlled disarrangement in the balance between male and female maturation tendencies.

General neurotic behavior as the result of primary maturational disturbances was studied by W. Kretschmer (83), the son of E. Kretschmer, a distinguished constitutional typologist of the German school of psychiatry (82). The latter contributed a very interesting description of asynchronic

puberty disorders as the basis of a "heboid" reaction syndrome and thereby provided some personalized evidence not only for the genetically oriented theory of Juda, reviewed in another section, but also for the superior and lasting quality of his own life work. In the study (83) by the junior member of the family, 40 of the 50 neurotic female patients of the psychiatric clinic showed evidence of genital underdevelopment, with irregularities in the age of menarche and various signs of retardation in the development of secondary sex characteristics (underdeveloped breasts, lack of sexual desire). In a control group of women attending the gynecological clinic, only 6.7 per cent of women of comparable age revealed symptoms of genital underdevelopment. The frequent combination of physical immaturity and neurotic symptomatology was ascribed to a common disturbance of maturational processes on a constitutional basis. Compulsive neurotics, psychopathic personalities, and emotionally disturbed patients with situational conflicts were excluded from the psychiatric series.

The genetic background factors of the compulsive behavior pattern were re-investigated by Rüdin (118), the younger member of another outstanding parent-child combination in the ranks of German psychiatric genetics. In this instance, the father was prevented by his death, on October 22, 1952, from making further personal contributions to the progress of the scientific specialty, of which he had been one of the founders.

In the impressive Munich series of 130 clinic and hospital patients with compulsive symptoms (55 males, 75 females), increases over general population figures were observed not only with respect to the intelligence level, social status, and celibacy rate of the index cases, but also with respect to familial tendencies to compulsive disease (parents 4.6 per cent, sibs 2.3 per cent, children 1.3 per cent) and suicide (parents 3.5 per cent, sibs 2.1 per cent, children 7.7 per cent). The family incidence of schizophrenia (sibs 1.5 per cent, children 2.5 per cent) and manic-depressive psychosis (parents 3.0 per cent, grandparents 1.3 per cent) was also slightly increased, but not sufficiently to establish a direct relationship between the compulsive reaction pattern and either of the major psychoses. Instead, the genetic predisposition to the compulsive reaction syndrome was assumed by the investigator to be associated with a polygenically determined primary dysfunction, showing continuous variation and brought into play by a variety of precipitating factors tending to intensify the constitutional inadequacy. Comprehensive reviews of the constitutional, physiological, and morphological mechanisms, which may play an important part in phobic or compulsive stress symptom formation, were presented by Gellhorn (38, 39) Graham (44), James (59), and Panse (104).

The challenging attempts in the psychoanalytic literature to extend the groundwork of psychodynamic theory to the dynamics of gene-controlled phenomena included Rado's concept of an integrative pleasure deficiency (110), which may become so pronounced as to result in complete adaptive incompetence (schizophrenia), and Székely's genetically oriented theory of

fears originating in early childhood (137). The latter theory was based on the assumption that "the descriptive concordance between (a) the fear- (and smile-) releasing Gestalt in the first year of man's life and (b) the animal key stimulus is the expression of a genetic relation between the two . . . and that the human fear-releaser is a phylogenetic survival from the animal-enemy-schema."

PSYCHOSES

While psychotic behavior patterns are precipitated by an unfavorable environment in a certain number of vulnerable individuals and frequently have an adverse effect on the social status of the family, it was re-stated by many investigators, especially by Bleuler (11), Böök (12), Kallmann (69), Kehrer (77), Kemp (80), Rêgo & Koch (115), Schulz (122), and Slater (130), that severe mental disorder occurs in all segments of the human population, regardless of social, economic, or cultural status. It is observed in slums as well as in royal families, and its incidence variations from population to population are surprisingly limited, as was again shown by Böök (12) for an isolated North-Swedish group of people living far north of the Arctic Circle.

The investigated population (4,584 in 1900 and 8,981 in 1949) consisted largely of small farmers and lumbermen, who had a low degree of mobility and mostly were descendants of people who had migrated into the forested area between 1650 and 1730. The incidence of marriages between first cousins among all marriages existing in 1947 was 2.2 per cent, thus marking the population as an isolate. The total registration of psychotic and mentally defective patients amounted to 364 individuals who belonged to 285 parent-sibship combinations. A series of 240 sibships could be consolidated into one large pedigree complex and was traceable to 31 ancestral pairs who had lived between 1700 and 1750. With correction for excess mortality, the general morbidity risk for schizophrenia in the area was estimated at 2 to 3 per cent (crude incidence for males 1.04 per cent, for females 0.84 per cent). By contrast, the incidence of manic-depressive psychosis was very low.

In line with the genetic hypothesis (Kallmann), psychotic behavior disorders were found everywhere to follow familial patterns of distribution. Hence, they would not be adequately explained either by a universal species vulnerability or by such general emotional group experiences as may be related to different child-rearing practices, dominant cultural value systems, or other particular social class variables. If it can be demonstrated that the given disorders affect only some members of some families under nearly equal conditions of stress, the key to persisting obscurities in their etiology should be sought in certain specific disarrangements of those maturational, integrative, or regulative functions which determine the adaptive capacities of a human organism. The statistical requirements to be met by genetic data were formulated as follows: (a) that the tendency to develop a severe psychosis be specific in nature; (b) that it be shown to follow a statistically

predictable pattern of distribution; and (c) that it should increase in proportion to the degree of blood relationship to a family member affected by the given type of psychosis.

Irrespective of the validity of data substantiating the theory of ecological drift before or after the occurrence of mental illness (direct relationship between the existential conditions of a population group and the number of mentally disordered persons in that group), the genetic hypothesis was recognized by various ecologically oriented investigators as not necessarily being inconsistent with psychological or sociological explanations of the etiology of severe behavior disorders. According to Clausen & Kohn (21), for instance, in order to fit the genetic hypothesis to the observation that schizophrenia may have a harmful effect on the social status of a family or on the quality of its interpersonal relationships, it would be necessary only to assume that a substantial proportion of the parents and grandparents of schizophrenics were themselves reduced in socio-economic status or in their empathy potentials, by virtue of the same genetic factor which presumably produced an innate vulnerability to schizophrenia in some members of subsequent generations. Over a number of generations one might well anticipate substantial differentials in incidence rates for schizophrenics in areas of differing social climate, or in the distribution of personality traits related to the reproductive, adjustive, or protective capacities of prospective parents. It was generally realized, however, that the genetic hypothesis would not really depend on any of these variables of psychological or sociological interest.

Specificity of major psychoses.—The crucial theory of specific genetic mechanisms for the two major types of psychosis, schizophrenia and manic-depressive psychosis, was confirmed by the family studies of Böök (12), Elsässer (28), and Stenstedt (135) as well as by Kallmann's and Slater's twin data (69, 129). The latter revealed neither a twin pair with a schizophrenic psychosis in one member and a manic-depressive psychosis in the other, nor a single manic-depressive index family with an authentic case of schizophrenia among the parents and siblings of the index cases. The observed trend was toward an increased incidence of schizophrenia in the relatives of schizophrenic index cases, and toward an increase of manic-depressive psychosis in the relatives of manic-depressive index cases.

Elsässer's study (28) dealt with the distribution of psychoses in sibships with two psychotic parents and, because of the extreme infrequency of fertile matings of this kind, was beset by various methodological difficulties. The total sample consisted of 134 single-born sibships, but it required six subdivisions, four of which comprised matings between one schizophrenic and one manic-depressive parent (19) or matings characterized by an "atypical" psychosis in one or both mates (61). Only the remaining parental combinations were formed by two clearly schizophrenic (34) or two clearly manic-depressive (20) parents.

On the whole, the observed psychosis rates for the offspring were some-

what lower than the ones reported by other investigators. However, falling generally between 30 and 40 per cent, they were far above the expectancy of persons with only one schizophrenic or manic-depressive parent. Most striking was the observation that the psychoses of the children in all mating groups followed strictly the pattern set by their parents, thus corroborating Kallmann's finding (73) that "whenever there were any diagnostic difficulties with respect to the symptomatology of one member of a psychotic index pair distinguished by monozygosity, they inevitably recurred in the classification of the other twin partner." In general, Kallmann concluded from his twin data that previous reports of frequent familial concurrence of schizophrenic and manic-depressive psychoses had largely been a result of, and surely would be most easily explained by, what he called "the inevitableness of diagnostic inconsistencies."

Manic-depressive psychosis.—The potentialities for a cyclic psychosis, when adequately defined, were assumed to rest on the specific ability of a group of vulnerable persons to exceed the normal range of emotional responses with extreme but self-limited mood alterations (73). The morphological substrate of the given dysfunction was thought to be associated with a subtle disturbance in a neurohormonal control mechanism which ordinarily protects man's emotional life from harmful extremes of affective responses. The regulatory instability produced by this dysfunction seemed to be correlated with the genetic factors for gout and diabetes, with a relatively high degree of resistance to tuberculosis, and with a general tendency to obesity and cardiovascular disorders. However, the regrettable incompleteness of present knowledge regarding the biochemical constituents of the underlying genotype as well as the range of its compensatory adaptiveness was stressed by all investigators.

As to the mode of inheritance classifying the potential vulnerability of cyclically reacting persons, there was general agreement on the theory of simple, autosomal dominance with incomplete penetrance. Consistent differences were observed between the general population rate, which usually does not exceed 0.4 per cent, and the morbidity rates for relatives of cyclic index cases.

In Stenstedt's study (135) which was based on a series of 216 hospitalized manic-depressive index cases drawn from the population of an isolated North-Swedish district, the total expectancy of manic-depressive psychosis among the sibs, parents, and children of the index cases was found to approximate 15 per cent, "if the results of previous investigations are considered together with the results of the present study." The 72 cyclic psychoses observed in the proband families were equally distributed between the sexes, but the diagnostic criteria used appeared rather liberal.

Of particular interest was the observation that even in a rural population, manic-depressives had a lower marriage rate than the general population. Their mortality was found to be increased, while the fertility of married patients failed to reveal a significant decrease. The social status of the sibs

and children of cyclic index cases "did not deviate remarkably from the average population."

The homogeneity of the manic-depressive genotype was demonstrated by the absence of morbidity risk differences between the sibs of male and female probands, and between the sibs of probands with only one manic and only one depressive episode. However, the morbidity risk for the sibs of "purely" cyclic probands was found to be higher than that for the sibs of probands distinguished by some "disturbing elements" such as peculiar personality traits or schizophrenia-like symptoms. Certain environmental adversities in childhood were interpreted by the investigator as having the tendency to "increase the risk for the psychosis among the siblings of the probands." From the standpoint of diagnostic classification, it probably was not without significance that 53 per cent of all the cyclic cases included in the analysis had only one psychotic attack. It was in accordance with expectation, however, that the first attack observed was a depression in 83 per cent of the cases.

According to American twin family data (73), comparative sibship rates vary from 16.7 per cent for the half-sibs to 22.7 per cent for the sibs, 25.5 per cent for two-egg co-twins, and to nearly 100 per cent for one-egg twin partners. The corresponding rate for the parents was found to be 23.4 per cent. The relative infrequency of the disorder in modern populations was thought to be attributable chiefly to factors of selection, reducing the reproductive rate of the carriers without affecting the social level of their families. At least, there was no evidence for a social decline of the magnitude typical of schizophrenic family units.

Schizophrenia.—As regards the basic dysfunction in the etiology of a schizophrenic psychosis, the diverse inadequacies in the adaptive capacity of potentially vulnerable persons were believed by Kallmann (69) to be best explained by the effect of a recessive unit factor, producing the specific potentials of disordered behavior patterns by a metabolic disturbance in the enzymatic range. The consequent vulnerability was shown by Funkenstein's group (33 to 37, 93, 94) to be correlated with altered reactivity to autonomic drugs, especially methacholine (mecholy), which was used as a detector of epinephrine or nor-epinephrine.

Evidence for a qualitative abnormality of adrenal responsivity in schizophrenics was presented especially by Hoagland (51, 52), but Selye (125, 126) considered the available data as insufficient to substantiate the assumption of an adrenal substrate for schizophrenia. According to Alexander (2), Fischer (32), and Hoffer *et al.* (53), there was reason to believe that the gene-specific deficiency in schizophrenia will eventually be traced either to changes in the production of a still hypothetical anti-epinephrine factor or to adrenochrome, a newly isolated hallucinogen produced by the human organism and related chemically to such drugs as epinephrine, mescaline, and lysergic acid. However, Braceland (15) may have been correct in stressing that "whether adrenal aberration is the cause or the result of the mental disorder

with which it is allegedly associated, remains to be established." In any case, it was assumed by Bender (9) and Kanner (54) that the symptomatology observed in childhood schizophrenia (specific maturational lags, early infantile autism, familial trend toward detached, obsessive, mechanical living) would be traceable to essentially the same gene-controlled deficiency as the one postulated for the main group of schizophrenic psychoses.

In Kallmann's opinion, the specific homogeneity of the genotype was indicated by the observation of different types of symptom formation not only in one-egg twin partners and other members of the same family unit, but also in affected individuals at different times. The theory of a recessive mode of inheritance was believed by him to be supported particularly by the distribution of the disorder in affected families (likely to be subject to factors of selection in mating and reproduction) and by an excess of consanguineous marriages among the parents of schizophrenics, while Böök (12) and others favored the theory of a simple dominant gene with a heterozygous penetrance of about 20 per cent and a homozygous penetrance of nearly 100 per cent. The frequency of the gene in the general population was estimated by Böök at approximately 7 per cent, and 6 to 7 per cent of the schizophrenic population were assumed by him to be new mutations. Of course, since it was implied by Kallmann's theory of recessiveness that neither the pathological gene in the homozygote nor the dominant factor for health in the heterozygote has a 100-per cent-expressivity rate in the two types of carrier, there was really little difference between the theories of dominance and recessiveness in this instance.

The long-established fact that the incidence of schizophrenia in normal populations and proband families differs significantly remained undisputed. In the absence of special conditions of inbreeding, the general expectancy was shown to be no lower than 0.7 and no higher than 0.9 per cent. The corresponding rate for relatives of schizophrenics appeared to vary from 7.1 per cent for half-sibs, through about 14 per cent for full sibs and two-egg co-twins, to 86.2 per cent for one-egg twin partners. The observed expectancy of children of one schizophrenic parent was 16.4 per cent, and the incidence of the disease in the parents of schizophrenics, 9.3 per cent. Next to one-egg co-twins, the children of two schizophrenic parents were found by all investigators to have the highest expectancy of a schizophrenic type of psychosis.

The pronounced variability in the clinical expressions of the genotypical dysfunction was assumed to depend on the interaction of general constitutional modifiers and those precipitating outside factors arising from uncontrolled imperfections in the structure of modern societies (69). Measurable correlates of this graded deficiency in resistance to the primary dysfunction were seen in the capacity for mobilizing effective mesodermal defense reactions, in the compensatory power of the athletic component of physique, and in the ability to maintain a stabilized level of body weight. Heterozygous carriers were thought to have either a schizoid or a normal type of personality while living under ordinary conditions.

Involuntal psychosis.—Relatives of persons affected by an involuntal

type of psychosis were reported to be distinguished by an increase in both involuntional and schizophrenic psychoses (69). However, only the increase in involuntional psychoses was pronounced as was indicated by expectancy rates varying from 6.0 per cent for the sibs and two-egg co-twins to 60.9 per cent for the one-egg twin partners of involuntional index cases. The schizophrenia rates for the same group of families, although somewhat above those for the general population, were considerably lower than the rates observed among the relatives of schizophrenics. The incidence of manic-depressive psychosis in these families was not increased at all. In other words, there was no evidence of a genetic relationship between the entity of manic-depressive psychosis and the symptomatology of involuntional melancholia or similar nonperiodic forms of depressive behavior in the involuntional period.

The genetic inference was that the development of an involuntional psychosis requires not only the capacity for survival into later years without psychosis, but also a certain form of emotional instability which reaches the threshold of a psychosis only under the age-specific impact of cumulative emotional strain and insecurity, that is, in combination with progressive impairment of general adaptability. The clinical specifications of this type of instability appeared to be met especially by the characteristics of schizoid personality traits, genetically identified as the least resistant phenotypes of heterozygous carriers of the schizophrenic genotype. This theory implied that the principal genetic derivation of involuntional psychosis is from an indirect relationship to the entity of schizophrenia, and not from a specific type of predisposition producing a particular impairment of the adjustive plasticity of aging persons.

Presenile and senile psychoses.—Theories of simple dominance or recessiveness were considered equally inconclusive with respect to mental disorders peculiar to the senium (69), with the possible exception of some specific types of presenile brain atrophy. Generally speaking, the etiology of the usual forms of a senile psychosis was assumed to be based on an interplay of complex determining factors including age-susceptible personality traits, reduced adaptive plasticity, and those gene-specific biochemical phenomena which control growth and decline.

In the group of presenile brain atrophies, Sjögren *et al.* (128) considered the hypothesis of a dominant gene influenced by other modifying genes as more plausible in Pick's disease than in Alzheimer's disease. In this study of 80 family units, the disease expectancy of parents and sibs was found to be higher in the families of Pick's than of Alzheimer's cases (19 and 6.8 against 10 and 3.8 per cent, respectively). In spite of a preponderance of female patients, the theory of partial or complete sex-linkage was not substantiated.

NEUROLOGICAL DISORDERS AND MENTAL DEFICIENCY STATES

In order to promote interdisciplinary interest in the causal analysis of the factors and forces involved in the genesis and heredity, growth, differentiation, and maturation as well as the maintenance, regeneration, and possible adjustments of the nervous system, including the neural basis of

behavior, a thorough study of the methods and objectives of neurobiology (genetic neurology) was made by Bard, Bronck, Clark, Lashley, Penfield and Weiss in a policy-setting statement issued by the Committee on Neurobiology of the National Research Council (140). The nervous system with its millions of neurone circuits, through which "the body and the mind alike are controlled and sensorimotor mechanisms are coordinated . . . in such a way that conscious thought and direction of human activity are possible," was described as "the meeting place of body and soul," and the future history of mankind was believed " . . . to depend in some degree upon more effective study of this relationship."

Detailed reports on the pressing and highly diversified genetic problems of primarily neurological disturbances were included in the books listed previously, especially those by Kallmann (69), Kemp (80), and Sorsby (132), and in the symposia of the Association for Research in Nervous and Mental Disease (54) and the Milbank Memorial Fund (139).

Linkage.—In an excellent monograph on human linkage data, Mohr (95) reported on linkage relations among sex, seven blood group systems, and taste sensitivity to phenylthiourea, and between these traits and such graded characters or inherited disorders as fingerprints, eye color, tongue curling, cataract, aniridia, Thomsen's disease, Huntington's chorea, and osteogenesis imperfecta. Conclusive evidence was obtained for linkage between the Lewis and Lutheran blood group systems, while the assumed linkage relations between (a) eye color and Duffy blood groups, (b) myotonia and Lewis-Lutheran blood groups, and (c) cataract and P blood groups were less clearly substantiated. The frequency of crossing over between the Lewis and Lutheran systems was found to approximate 8 per cent.

Convulsive disease.—The uncertainties in the available information about the genetic aspects of epilepsy, specified by Alström (4), Kallmann (69), Kemp (80), and Pratt (107), remained largely unresolved. Lennox & Jolly (54) presented new clinical, psychometric, and electroencephalographic data on a series of 155 twins, both monozygotic and dizygotic, supporting their belief in the important part played by genetic factors (presumably causing a specific type of metabolic disturbance) in the etiology of "idiopathic epilepsy." Nevertheless, Alström remained unconvinced of the statistical validity of these data, expressing his doubts in a number of lectures and personal communications. In his opinion, only a small fraction of epileptics owe their disease to heredity.

Mongolism.—Similar etiological obscurities persisted with respect to Mongolism, another comparatively common, severe, and much-disputed type of disorder accounting for 5 to 10 per cent of all mental defectives. According to nongenetic theories supported especially by Benda (8), Ingalls (139), and Øster (103), the condition was assumed to originate in the fetus before the third month of pregnancy as a result of varying toxic influences arising from the internal (uterine) environment. Causal connections were postulated particularly with the mother's advanced age and most recently

reaffirmed by Øster's extensive study of 526 Danish cases of Mongoloid idiocy and their families (9,858 relatives). Apart from an increased familial incidence of epilepsy and deaf-mutism and a low parental consanguinity rate, Øster observed six sibships with two Mongoloid defectives ("statistically expected"), 10 other instances with one or more affected relatives ("also in accordance with statistical expectation"), and a considerable number of primiparous mothers who had been over 35 years of age at the conception of their Mongoloid child. He ascribed the condition to "exogenous factors related to the mother's depressed reproductive faculty," but Jervis (62, 63) doubted that the etiological significance of these factors had been conclusively substantiated. In line with the genetic hypothesis suggested by Penrose (105) and based especially on twin data, fetal susceptibility to Mongolism was assumed to be a simple recessive trait with a gene frequency as high as 1 in 5 but with a penetrance as low as 1 in 27, bringing the incidence of the trait to about 1 in 600 births. Maternal age factors would control the expression of the trait, and penetrance would tend to be greater when the mother is herself homozygous for the gene and hence potentially, but not actually, a Mongoloid.

Congenital malformations and other defects.—Harelip and cleft palate, classified by Kemp (80) as genetically determined according to the principles of "conditioned dominance with sex limitation to males and considerably less manifestation of the heterozygous than of the homozygous form," were found by MacMahon & McKeown (91) to be causally related to advanced maternal age, too. In a survey of 285 cases drawn from a total of 218,693 babies, born in Birmingham in the years 1940 to 1950, a careful analysis showed harelip with or without cleft palate to have been nearly four times as frequent in the offspring of mothers in the age range 38 years and over as in those of young mothers. However, that maternal environment and genetic constitution may interact throughout prenatal development (so that environmental factors producing an anomaly in one individual may be harmless to one with a different heredity) was effectively demonstrated by Ingalls *et al.* (57) when experiments in mouse embryos of different strains revealed strain-specific differences in susceptibility to hypoxia-induced malformations.

Borle's (14) theory that some cases of hydrocephaly may be genetically determined malformations caused by a stricture of the aqueduct of Sylvius was supported by the observation of a concordant pair of one-egg twins, who had an equally affected brother, as well as by animal experiments. Convincing evidence for the genetic origin of Sturge-Weber's disease and certain forms of primary microcephaly (an apparently recessive condition) was presented by Koch (81) and Böök *et al.* (13), respectively.

Allergic disease.—In the group of psychosomatic disorders assumed to be attributable to multiple factors of causation (both physical and emotional), familial types of allergic hypersensitiveness were shown by several investigators to vary considerably in the intensity of the precipitated antibody-anti-

gen reaction as well as in the period of manifestation of the given susceptibility. With respect to bronchial asthma, Schwartz (124) confirmed the theory of a specific genotype, apparently autosomal dominant with 40 per cent penetrance and somehow interrelated with the genetic background factors for vasomotor rhinitis, prurigo, and hay fever. His conclusions were based on a study of 441 index and control families and received some support from the animal experiments of Ratner (111) and Chase (20). However, they were questioned by Ratner & Silberman (112) on methodological grounds.

Disturbed sexual development.—While interesting pedigree studies were reported by Best & Münch (10) in relation to Laurence-Moon-Biedl's syndrome (combining the features of dystrophia adiposo-genitalis with retinitis pigmentosa and polydactylism) and by Jacobsen & Macklin (58) in relation to a hereditary type of sexual precocity (one family with 27 affected males), Sohval & Soffer (131) described what they assumed to be a new gene-controlled syndrome, characterized by a moderate degree of androgen deficiency, small testes, aspermatogenesis, gynecomastia, and increased urinary gonadotropins. The tendency to familial occurrence in some cases, displaying the typical hypogonadal symptomatology of Fröhlich's syndrome, was confirmed by Jenny (60) in an interesting sibship (six sibs showing "girdle type" obesity, sexual underdevelopment or mental defect in varying combinations) and, because of the observed consanguinity of the parents, was classified as a recessive trait. A similar family constellation (Bardet-Biedl's syndrome in the three children of a cousin marriage) was placed on record by Grebe (45) and interpreted as evidence for a hereditary type of "diencephalosis" (disturbance in the regulatory center located in the diencephalon). The possible significance of gene-controlled mechanisms in ordinary types of obesity associated with early menarche and disproportional preadolescent growth was stressed by Schopbach & Angel (120) and explained on the basis of a "sensitized hypothalamic appetite mechanism." Concordance as to both adenomatous goiter and Morgagni's syndrome (obesity, hirsutism, and hyperostosis frontalis interna) was observed by Aschner *et al.* (5) in a one-egg pair of schizophrenic twin sisters.

Extrapyramidal system affections.—An apparently gene-specific (recessive) deficiency in ceruloplasmin (plasma protein) was discovered by Scheinberg & Gitlin (119) as the probable basis of Wilson's disease, a familial disorder distinguished by cirrhosis of the liver, bilateral changes in the lenticular nucleus, and abnormalities in copper metabolism. Interesting family data with respect to this condition (hepatolenticular degeneration) were reported by Sullivan *et al.* (136) and Heuyer *et al.* (50). The former study dealt with five affected sibs including one pair of twins, and the latter provided evidence of the disease in four sibs.

Valuable support for the theory of irregular dominance (with a manifestation rate of about 50 per cent) in the familial type of Parkinson's disease was presented by Pintus & Sarteschi (106) who observed the condition in

eight members of a sibship of 17 persons. Equally important were the results of a study conducted by Leese *et al.* (87) in a large family affected by Huntington's chorea (single-dominant trait).

Other nervous system affections.—Both tragic and enlightening were the comprehensive reports prepared by Schut (54) and Schut & Böök (123) on the classification and variable modes of transmission of the hereditary ataxias. Because of the close association of the senior author with a large family afflicted with the condition, his efforts to detect preclinical signs "among individuals destined to inherit hereditary ataxia" were in the finest Hippocratic tradition. With respect to the late-developing forms of amaurotic idiocy, generally recognized as belonging to the group of cerebral lipidoses, Rayner's (113) observation of vacuolized lymphocytes in phenotypically normal relatives of several juvenile cases raised the hope of having opened a new avenue to the detection of heterozygous carriers. From the standpoint of genetics, as related to an understanding of selective population trends, Hablützel's (46) study of an isolated Swiss community with a very high prevalence of intellectual deficiencies was another notable contribution.

Important neuropathological findings in rare familial syndromes of genetic interest were published by Kantarjian & DeJong (75) in relation to primary amyloidosis with nervous system involvement (relentlessly progressive, widespread disease in three members of one family) and by Von Braunmühl (16) with respect to an unusual condition combining features of Friedreich's and Pierre Marie's forms of spinocerebellar ataxia (two sisters).

No evidence for a possible etiologic significance of genetic factors in ordinary types of brain tumor emerged from Kuhnen's (84) study of seven pairs of twins, three of whom were monozygotic. All pairs were found to be discordant.

Metabolic deficiency states.—Excellent reviews of the almost entirely recessively inherited biochemical alterations identified as the potential causes of mental impairment were presented by Herndon (54) as well as by Jervis (54, 61, 139). Being in agreement on all essential points, they divided the deficiencies into the following four groups of disorders in metabolism: lipids, amino acids, carbohydrates, and pigments.

Amaurotic idiocy was considered the prototype of the first group, consisting mainly of the cerebral lipidoses. Some doubt was expressed by Herndon as to whether gargoylism should be classified as a primary lipidosis. In any case, he believed that the existence of two genetically distinct types (a simple recessive and a sex-linked recessive variety) was indicated by a consistent excess of male patients.

Phenylpyruvic oligophrenia was considered the most important disorder associated with an inborn error in the metabolism of amino acids. Of the other two groups, Von Gierke's disease (aberrant metabolism of carbohydrates) and Hallervorden-Spatz's disease (altered metabolism of pigments) were thought to be most typical.

Undifferentiated type of mental deficiency.—Statistical estimates and

classificatory explanations of the group of undifferentiated high-grade deficiencies continued to vary considerably. Jervis (61, 64) agreed with those assuming that this group, unlike that of the low-grade mental defectives, represents the tail of the normal distribution of intelligence and, therefore, is an integral part of the general population. Accepting the interaction of cultural factors, Jervis stated that the same genetic mechanisms productive of normal intelligence operate in the high grade undifferentiated defective.

The part played by heredity in the production of this type of high grade deficiency was shown by Roberts (116) to be determined chiefly by polygenes.

SCIENTIFIC OUTLOOK

The therapeutic potentials of genetic research were stressed by many fieldworkers to combat the rationalized notion that genetically determined disorders are "unalterable finished entities." This still rather popular belief was traced by Sorsby (132) to the "historical accident" that hereditary factors were most clearly recognized in congenital anomalies. Actually, considerable progress was made in the last few years in an effort to demonstrate that many gene-specific disorders are neither congenital nor unchangeable, and that even an affection present at birth does not arise as a finished entity.

Hence, the ultimate goal of genetic research in man was seen in opening new pathways toward causally directed methods of therapy, capable of counteracting the pathologically disruptive effect of a particular mutant gene either directly or with some substitutional procedure. The steadily expanding field of biochemical genetics was credited with furnishing a realistic basis for the hope that "complete analysis of primary gene action may soon emerge as one of the most promising scientific developments of our time" (69). That the early detection of "carriers" of hereditary traits may be expected to lead to fundamental contributions from the standpoint of understanding the mechanism of gene action (particularly by providing material for the study of "minimal" gene effects) was effectively shown by Neel (99, 100).

Although complete cure of gene-dependent mental disorder was identified as the ultimate objective of psychiatric genetics, it was widely realized that severe deficiency states should be considered not only in terms of active therapy, but also in terms of empirical prediction and tentative prevention. Because of the rejection of compulsory methods of public health planning by democratic societies, it was thought to be a mandatory obligation for American public health authorities to make adequate provision for expert guidance on problems of marriage, parenthood, and inheritance where it was needed and sought voluntarily by morally responsible people (66, 68, 69). In order to be able to deal with such guidance activities under conditions of severe maladjustment, it was considered essential for psychiatrists and clinical psychologists either to have some basic genetic training of their own or to take an active interest in the training and professional conduct of specialized guidance workers, expected to render advice in matters of family relations and heredity in the presence of gross emotional distress.

For the sake of real progress in coping with the complex problems of psychopathology, emphasis was placed by many writers on the need for a well-balanced approach "delving into the substratum of gene action as well as into the subconscious." In the slow process of synthesizing innumerable fragmentary pieces of scientific information about human behavior, the least grievous mistake was believed to come from treating even respectable behavioral theories as not yet final, that is, as a policy and not as a creed. If investigators, using ecologically distinguishable stepping stones in the study of the etiology of mental illness, wish to cross the stream rather than be left stranded in the middle of it, they will have to realize, as Clausen & Kohn (21) put it, that "the stream may be deeper than has been recognized." Equally challenging views were expressed by Barber (6), Brown (17), Dunn (27), and Lenz (88).

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THEORY AND TECHNIQUES OF ASSESSMENT^{1,2}

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INTRODUCTION

One of the difficulties encountered in writing a review of assessment is that assessment as a distinctive area scarcely exists in its own right. Hence the first review (78) covered the topics of psychodiagnostics, the diagnostic aspects of counseling, and the methodological aspects of personnel selection. As Kelly (78) indicated, these somewhat disparate applied fields have suffered in their development from ignorance of the other fields and, the writers would contend, have also suffered from the somewhat uncritical acceptance of the methods and techniques common to the others.

The problem common to all approaches to assessment is the problem of categorizing individuals for the purpose of making differential predictions of behavior. The problem of prediction is one that is common to all psychologists. In assessment, however, accuracy of prediction for the purpose at hand is the end-in-view whereas for the psychologist acting as a scientist, the prediction of behavior is but a step in uncovering a law or an intervening variable; the prediction in itself is of no inherent interest. Assessment therefore involves prediction in the service of nonscientific goals although the assessment process may employ scientific methods and may yield scientific knowledge. One may distinguish psychological assessment, then, from general psychology and its sub-areas on the basis of objectives. We usually assess because we want to select good butter-wrappers, or sales people, or good students, or because we want to aid in the process of distributing individuals to satisfying socioeconomic environments or because we want to help them or help others to help them to attain better "mental health" or personal adjustment.

It should be noted that assessment involves the relationship between two samples of behavior selected rather arbitrarily from the total course of the individual's life. Thus high school grades may be used to predict college grades, which in turn might be viewed as predictors of vocational success. Diagnostic protocols may be used to predict the course of psychotherapy, which itself might be the basis for prognosis of subsequent adjustment.

¹ The survey of the literature pertaining to this review was completed in May, 1954.

² The following abbreviations are used in this chapter: MMPI (Minnesota Multiphasic Personality Inventory); TAT (Thematic Apperception Test).

³ The authors are deeply indebted to Mr. Fred Zimring who rendered us invaluable assistance and found many more references than we could include here, and to the Social Science Research Committee of The University of Chicago which made this assistance possible.

Broadly speaking we may distinguish two types of assessment. The first is assessment for the general purpose of predicting efficiency of performance. This kind of assessment has usually proceeded by testing for aptitude, intelligence, and achievement and has relied on the classical mental test methods which were developed largely to meet the problems of such testing.

In the second type of assessment, the concern has been with evaluating the individual for the purpose of identifying and limiting treatment possibilities aimed at restoring or arresting deterioration in personal adjustment. In this type of assessment it was soon discovered that the methods developed for use in the first type did not apply, and out of the gropings of the clinicians rose what Kelly (78) referred to as the organismic hypothesis. This hypothesis as it relates to the process of assessment assigns the integration of data and prediction to the assessor in the belief that he can take into account the disparate and formally incommensurable data available to him in the form of test scores, history, and interviews and improve to a significant degree the predictions capable of being made from formally commensurable data.

The organismic hypothesis also involves the notion that the data available to the assessor must be ordered relative to their standing within the individual. This is to say that predictions of behavior for assessment purposes must take into account sets of variables in terms of their relative magnitude within the individual. This is precisely the domain which "objective" or psychometric procedures have not penetrated. To the writers it seems that the issues have been obscured by what appear to be almost ideological controversies between what may be conveniently called the clinically oriented and the personnel oriented. The contradictions become clearer when one considers vocational counseling, that half-way house between the clinical and personnel orientations. Historically this was oriented toward distributing information to individuals about their vocational and educational surround. The success of the testing movement led vocational counselors to use test scores (including biographical data treated as test scores) and regression equations for individual predictions. Still later the test information was supplemented by evaluations of personality, and the vocational counselor found within himself the conflict existing between the two orientations described above. It is perhaps fair to say that the resolution of this conflict typically took the form of placing heavy reliance on the organismic hypothesis. For example, with a student for whom predictions failed to be verified, some modification of the evaluation was made on the basis of the organismic hypothesis.

The organismic hypothesis in assessment work actually arose out of the attempt of clinicians to deal with data not currently amenable to objective methods. Consequently the various clinical approaches to assessment were of necessity subjective. However, the historical contiguity of subjective and clinical assessment, attributable to a dearth of suitable objective methods, does not logically imply a necessary contiguity. The hypothesis of the writers can be put quite simply: the metric suitable for the classical psychometric

approach to assessment is not sufficient for clinical assessment. The classical approach is concerned with differences between individuals on variables considered singly or in combinations, each combination being treated as a variable. Consequently the classical psychometric procedures utilize a metric over persons. For each individual a score is obtained which is usually scaled in terms of standard scores of the other individuals tested. A psychogram constructed on the basis of such scores reveals the profile of the individual in terms of variations from norms. The correlations between tests reveal to what extent the ordering of individuals on one test can predict the ordering of individuals on another test; it in no necessary sense reveals the ordering of behavior tendencies for the single individual with respect to the variables tapped by the tests. This point is so obvious as to be painful, but it is not one to which much attention has been explicitly accorded.

Yet another point must be made about the classical test metric over persons. Historically, psychometric procedures were developed for the assessment and scientific investigation of intelligence, aptitude, and achievement. It is precisely in this domain that the notion of difficulty of a test or of a test item is appropriate. In fact a domain may be defined by its common metric; e.g., the common metric of classical psychometrics is based on the concept of difficulty. Now the idea of difficulty has several interesting aspects. In determining the difficulty of an item for a defined population it is implicitly assumed that motivation is common and that it is optimal for performance. If motivation is not the same and if its strength is not roughly equal for all individuals, then the individual differences found will be attributable to motivational differences as well as to differences in ability. Thus difficulty applies to the upper limit of behavior under conditions of motivation which are optimal to performance.

The concept of difficulty does not provide a satisfactory common metric for personality measurement. The basic application of the difficulty concept, the power test, is not suitable in this domain because it would be untenable to assume that one item is higher than another on some general common metric.

The typical personality test constructed from a psychometric orientation utilizes sampling. A series of items, referring to feelings, attitudes, or behaviors, is developed, usually on *a priori* grounds. The score is taken to be the number of positive answers the subject gives, i.e., the number of different responses which are believed to be behavioral indications of the personality variable being measured. We know of no common metric (comparable to difficulty) on which personality items can be scaled.

Furthermore, in the measurement of a single variable, there is no norm for the set of the subject. On an aptitude or achievement test, both the examiner and the subject want the highest possible score to be obtained (i.e., want the subject to be scaled as high as possible on the difficulty metric). On a typical personality test, the variable being measured is unknown to the subject. The examiner seeks to elicit frank replies, at least as frank as those

made by the standardization population and by previous experimental groups. The subject, however, has no clear set. He has no incentive to be frank, except in a psychodiagnostic situation, and even then he may be unable to reveal certain facts even to himself.

Thus, in psychometric testing, performance is measured under relatively controlled conditions, which may rarely be replicated in day-to-day functioning. In personality testing, responses are elicited in a situation which is, to be sure, somewhat standardized but which is relatively uncontrolled. That is, the set may have important differences in meaning for the examiner and the subject. There is no explicit orientation comparable to that provided by the difficulty concept.

In objective personality testing the practices of the classical psychometrician have been followed and his methods have been applied with no more changes than necessity has required. This means that, although there is no fundamental ordering relation based on a common metric, the items are treated as though they indicated varying amounts of something. In addition a really formidable array of statistical assumptions must be made, and the treatment involves the assumption of a common metric but this assumption has no base in experimental operations. Furthermore, factoring tetrachoric correlations again yields a set of factors implicitly based on an ordering of persons on a metric which is assumed to exist only in the sense that it allows the tetrachorics to be used at all. This factoring does not, however, yield classes of, e.g., extroversion-introversion; instead it yields a set of presumed variables. The metric or continuum over which these variables vary is not specified.

The considerations outlined above are not mere technicalities. The results of personality testing have been sufficiently poor to indicate the need for critically examining the psychometric bases on which such tests have been constructed. It must be admitted that the results do not exhibit the degree of consistency obtained in the areas of ability, aptitude, and achievement. Some such consistency presumably would have been obtained if, in fact, some common, although unspecified, metric did underlie the personality tests.

Given this situation it appears that several alternatives are possible: (a) One or more common metrics can be identified which delineate domains within the broad area of personality. (b) Disregarding metric considerations, nominal classes of persons may be established, e.g., by latent structure analysis (87). If the classes are related to criteria, predictability is raised as contrasted with the situation in which the population has not been broken into classes. (c) A possibility related to the previous one is that once a set of classes has been found and individuals have been assigned to them, then the classes themselves may be put on some sort of scale, e.g., a scale based on a criterion. (d) Finally there is the possibility that subjective metrics might be established for individuals by using psychophysical methods or variants thereof (145). The operations would involve the discrimination and comparison of items on some specified dimension established by instruction, e.g.,

"Sort these items in terms of your preferences . . ." or "Sort these items according to their applicability to you. . . ." Such procedures may be applied by a subject, by an assessor, or both.

Our bias is that two of these possibilities should be exploited vigorously: the utilization of subjective metrics and the identification of nominal classes by sound methods.

Finally, assessment of individuals and assessment through variables require different methodologies. On the one hand it seems premature to restrict individual assessment to intuitive, preconscious operations on the part of the assessor and, on the other, it seems somewhat rigid to cling to classical psychometric procedures based on the common metric of difficulty.

METHODOLOGICAL CONTRIBUTIONS

In the opinion of the reviewers Stephenson's book, *The Study of Behavior* (145), is the most important single contribution of the year to assessment, although its scope transcends that of assessment. It is a most controversial book with respect to methodology and to technique. Arguing from a definite position with respect to the philosophy of science, Stephenson outlines a methodology which can be applied to the study of a single individual. Unlike the naturalistic approach of other advocates of the single case, this method involves "dependency analysis" in which the contributions of explicitly stated effects are tested. Theory can be utilized throughout the design and execution of each experimental study.

The operations of his *Q*-technique are systematically derived from his methodology. *Q*-technique has been loosely viewed as the factor analysis of correlations between people, and much controversy has been centered around this point. The technique is actually broader and more general. Thus Stephenson emphasizes the design of studies in which the analysis of variance is the statistical tool. Furthermore, he regards "the postulatory-dependency methodology of *Q* as 'open-ended'—the precise form of the analysis undertaken is determined by the experimental situation" (145, p. 40). Moreover, he demonstrates that the technique can be used with profit in many areas of psychology.

The reviewers consider that Stephenson's contributions to assessment include the following: a development and refinement of the forced-choice method; a persuasive argument for a new approach to the methodology of personality study; the delineation of an objective technique for quantifying highly subjective data; finally, and perhaps most significant, a careful exposition of the concept of a metric within persons, as opposed to the conventional metric on which both persons and items may be scaled. The full implications of a dimension of significance for the single individual are yet to be analyzed systematically or studied empirically.

A *Q*-technique study by Beck and his associates (10) exemplifies the use of the method in isolating classes of persons. First a list of clinical formulations about the schizophrenic condition were converted into brief behavioral

statements. For each, the Rorschach correlate or correlates, in terms of patterns or relative strengths, were specified. Then the behavioral statements were sorted by two psychologists on the basis of the Rorschach protocol and by a psychiatrist. The six patterns found by factor analysis are characterized from the behavioral statements. Although this study actually represents a first step in a complex area, it illustrates our contention that the use of subjective metrics may yield significant classes of persons.

However, some problems remain concerning *Q*-technique. For example, Edwards & Horst (32) point out that in many *Q*-sorts the social desirability of items should be controlled. On the other hand Butler (17) in deriving a personality test from Tolman's Psychological Model (151) found a within-person metric to be the most suitable for describing the generalization dimension of self.

In an important paper on profile analysis, a first version of which was reviewed by Kelly (78), Cronbach & Gleser (24) have advanced several criticisms against correlating *Q*-sorts. Granting the assumptions necessary for their mathematical model, the criticisms are sound. As they point out their model requires the assumption of equivalent intervals as between tests which in turn presupposes equal intervals within tests. Now clearly the forced sort operations of *Q*-sorting yield no more than a set of ranks with an arbitrary number of ties. The most appropriate measure of correspondence between *Q*-sorts is therefore the tau rank order coefficient (80) although the rho rank coefficient is very nearly a function of tau. Computation of a set of product moment correlations can be regarded as a set of operations producing a matrix of intercorrelations which will satisfactorily approximate the structure in a matrix of tau intercorrelations. The basic assumption need not be that of Cronbach & Gleser, which is usually unrealizable in actual practice. Rather it need only be that the final matrix of correlations be a satisfactory approximation to a set of rank order correlations. This assumption has been made by Thurstone (150, pp. 66-67) in developing multiple factor analysis. He points out, first, that test scores should be regarded as merely establishing a ranking of subjects and, second, that the usual transformations employed preserve the rank ordering of the subjects. Similarly the transformations employed in *Q*-technique preserve the rank ordering of items, and discussions of shape, elevation, and scatter as defined by Cronbach & Gleser are permissible only when one really believes that equivalence of units between different sorts actually obtains.

Another technique which should be important for assessment is Lazarsfeld's latent structure analysis (89). Latent structure analysis, which is mathematically related to factor analysis, may be viewed as a relatively rigorous method of determining the probabilities that respondents with given response patterns belong in given "latent classes." It can thus be considered as a type of pattern or configural analysis. Gibson in an interesting report (47) has shown how latent structure analysis may be viewed as factor analysis and has reduced the labor required to that comparable to a factor analysis. Therefore tests with 30 or 40 items may now be subjected to latent

structure analysis without undue expenditure of effort. In our opinion, however, latent structure analysis suffers from the fact that its various formulations do not permit of a simple structure solution. It is believed that it would be feasible to develop a type of latent structure analysis which would utilize the simple structure principle.

McQuitty (104) has also developed a configurational method of analyzing tests which are designed to measure personality integration. His method is based on the analysis of common elements correlations between pairs of items where the elements are the number of people endorsing a given alternative. His method has the merit that the common elements correlation as used requires only the presence and absence of responses without much in the way of assumptions. The restriction of the analysis to joint frequencies, however, implicitly requires that paired joint frequencies exhaust the information contained in the data. Since latent structure analysis indicates this is not the case, it seems that there is a certain necessary indeterminateness in the factors extracted by using the McQuitty approach. However, the impressive empirical results which have been reported suggest that the logical foundations of his method should receive extensive exploration.

Horst (70) investigated the problem of configural scoring of two and three items at a time and derived formulae for two and three item patterns for dichotomously scored items. He showed that configural scoring is a special case of a nonlinear combination of items and that his procedure could be generalized to the case of items with more than two possible responses. His procedure takes into account joint occurrences of two and three items and thus uses more information than the scoring of single items. It should be pointed out that his approach does not consider the constructs underlying the test.

Presumably the best configural scoring system would utilize the information contained in all of the 2^m response patterns in a test with m items. The multiplicity of response patterns raises the problem of the dimensionality of a test. Consider a test with two latent subclasses, one of which predicts a criterion and one of which does not. Now for each total response pattern there is a probability that the respondent comes from each one of the latent subclasses. It would seem in such a case that the response patterns should be weighted according to the probability that a respondent yielding a given response pattern belongs to the latent class that predicts the criterion.

The problem of configural scoring is one that deserves more attention than it has received. The reviewers are convinced that among the important bases for the relative lack of success of objective personality inventories is the fact that conventional scoring methods neglect much of the information contained in the test.

GENERAL REVIEWS

An excellent summary of the current status of assessment has been provided by Vernon in his *Personality Tests and Assessments* (158). His unbiased survey should be of particular value to workers in this country who may not

be familiar with recent British studies. It is a penetrating book which is unfortunately brief and condensed. Vernon emphasizes the necessity of utilizing more than a single approach to behavior sampling (e.g., ratings) in assessing personality. He also discusses the classical problem of observer bias in a refreshing way. He is realistically optimistic about the future even though he continually stresses the lack of definitive research findings in this area.

As Vernon suggests in his book, Eysenck's latest volume, *The Structure of Human Personality* (34), is largely an outline of the author's own work and views. In addition, he provides a review of much research literature, organized around various techniques, concepts, and methods. While his general conclusions about research methods are acceptable, his approach seems to be somewhat narrow. His review is primarily limited to the factorial studies, placing his bets on oblique structure and second-order factors. His own model of personality is based on three dimensions at the type level: neuroticism, extroversion-introversion, and psychoticism. While we grant the desirability of seeking pure tests of basic personality variables (if such can be found), we do not see how his taxonomy has contributed to personality theory, up to the present time. Unlike Vernon's handbook, this militant volume focuses almost wholly on method and on comparison of findings. Insufficient attention is paid to the quality of the raw data and to practical or theoretical implications. There are a number of bibliographical errors.

A less technical volume by Mace & Vernon offers a view of *Current Trends in British Psychology* (99). For the assessment field, it discusses what is being done rather than reporting new findings and formulations.

MacKinnon (102) summarizes past and current assessment programs. Two recent general reviews (9, 119) are also pertinent, as are chapters in Festinger & Katz (36). French (41) has offered a synthesis of factor studies in the personality domain, yielding 49 cross-identified factors. Schofield (135) has continued his annual categorization of research in clinical psychology and his discussion of contemporary trends. Questionnaires have been defended by Eysenck (33). Problems in the evaluation of counseling are discussed by Rushong (127).

CRITERION-ORIENTED STUDIES

Assessment involves two samples of behavior (a criterion and a test) and, at times, an assessor. In the remainder of this chapter papers are grouped according to which of the components seems primary. Thus a study may use a test primarily to predict or throw light on a criterion, or primarily to learn more about the test. Readers interested in a given test or criterion should examine all three sections.

Assessment of psychotherapy.—The steady flow of studies of psychotherapy, characteristic of the postwar years, has received its annual increment. Barron (8) developed a scale of 69 MMPI² items for the purpose of predicting response to psychotherapy. The scale clearly distinguished between groups of improved and unimproved patients. The differentiation held

up on cross-validating studies. The content of the scale suggested that "ego-strength" was the variable measured, and correlations of .38 and .41 with "general energy level" and "directed energy" respectively, as well as significant correlations with intelligence scales, confirmed the impression of the nature of the scale content. Barron concluded that ego-strength is a significant determinant of personality change in psychotherapy. As the author points out, it is not to be concluded that the improved patients were well-adjusted before therapy since their actual mental distress had been ascertained.

In another careful study by Barron (7) more test correlates of response to psychotherapy were reported. He found that the Paranoia scale of the MMPI differentiated significantly between improved and unimproved patients. When, however, clinicians attempted to predict outcome from MMPI profiles, the average accuracy was 62 per cent, a 12 per cent improvement over chance. He regards this as poor prediction. But when discussing reasons for lack of improvement Barron offers the following: patient too overbearing for therapist; male therapist proposed to give physical examination to sexually disturbed spinster; therapist responded with anger and anxiety to a seductive attempt by patient of same sex; Jewish therapist responded adversely to anti-Semitism on the part of his patient. Any study which relies on data obtained solely on patients requires the assumption that the therapists adequately carried out their therapeutic role. Otherwise an intolerable burden is placed on the predictive instruments. Hence, to the extent that the contributions of the therapists to lack of improvement is large, then to that extent prediction based on patient characteristics or patient behavior must be inaccurate.

Barron also found zero correlation between the Harris-Christiansen Rorschach Prognostic Index (61) and improvement, and that no determinant and no important ratio differentiated between the improved and unimproved groups. The Ethnocentrism scale correlated $-.64$ with improvement; with intelligence partialled out the correlation dropped to $-.34$.

Barron's study indicates rather clearly a major difficulty inherent in the study of psychotherapy and one which has received little attention, namely, the description and evaluation of the behavior of the therapist. The fact that the behavior of the therapist has not been controlled can only lead to errors of prediction. This is especially true in those cases in which the therapist has not maintained a consistently therapeutic role.

Gallagher (43) studied success in client-centered psychotherapy as measured by a multiple criterion. He found correlations with decrease in score on the Winne Neuroticism Scale and the Taylor Anxiety Scale of $.54$ ($P < .01$) for both tests. In another study of the same group of clients and using the same criterion, Gallagher (42) reported significant favorable changes on six MMPI scales and on a maladjustment scale derived from the MMPI. The maladjustment scale correlated $.58$, $.44$, and $.41$ respectively with the client's rating of therapy, a positive-negative feeling ratio derived from therapeutic

protocols, and the multiple criterion, which included the two preceding measures. He points out that the change in the MMPI was in general restricted to the mood or feeling scales. As in Barron's studies, Gallagher's presents little information on the influence of the therapist on the outcome of therapy except to state that the therapists used a client-centered approach and that most of them were inexperienced.

Peterson (117), using the same multiple criterion and the same subjects as Gallagher, compared indices derived from the Rorschach on the hypothesis that the greater the qualitative specification of a sign index, the more predictive of success it would be. The correlations of improvement-decrement scores with the criterion yielded the predicted order. However, none of the correlations was significant. Correlations of the criterion with normality-gain scores on the Rorschach signs did not yield the predicted order. Again the correlations were not significant. The author concludes that Rorschach signs incorporating qualitative specification do not contribute more to assessment than those more quantitative in nature. However, his actual results indicate that none of the sign indices used was related to the criterion to any significant extent. Consideration of his results with those of Barron and Gallagher raises again the question of the current projective versus the objective approaches to assessment.

Mindess (111), using Klopfer's Rorschach Prognostic Rating Scale, found that pretherapy total scores on the scale correlated .81 with a rating of adjustment after therapy. When psychotic patients were dropped from the group studied, the correlation dropped to .66. Kirkner *et al.* (82) studied the validity of the scale on a sample of 40 veterans. There were two psychotics in the group. Patients were classified as improved or unimproved. They found a phi coefficient of .44 between the sums of the raw scores and the criterion and one of .67 between the sums of the weighted scores and the criterion. Recalculation of the latter data by the reviewers using Spearman's rho (with correction for ties) yielded a correlation of .54. These results are all the more surprising since the therapists were third and fourth year clinical psychology trainees. The authors concluded that the scale should be a definite aid in evaluating a patient for acceptance in psychotherapy.

Johnson (77) reported on the effectiveness of the Rorschach Prognostic Rating Scale and the difference in standard scores between the Binet and Ravens' Progressive Matrices in predicting improvement and nonimprovement in play therapy. Twenty-one mentally and emotionally retarded children were studied. Of these, 15 were regarded as improved and 6 were regarded as not improved, the criteria being a composite of therapist judgment and social criteria. In general the treatment was longer for the improved than the unimproved children. For the improved group the direction of difference between the Binet and the Raven standard scores was in favor of the Raven for all cases; for 5 of the 6 unimproved children, the difference was in favor of the Binet. On the Prognostic Scale evaluation of improvement against change in score was statistically significant. Accuracy of prediction from

initial test, not tested by the author, was lower than the changes in scores indicated; of 10 children for whom prognosis was poor 6 were classified as unimproved and 4 were classified as improved. Hence, Binet-Raven difference scores were better predictors than Prognostic Rating Scales scores.

Lundin & Schpoont (96) published an interesting study in which the Prognostic Rating Scale scores were obtained before, during, and after psychotherapy, six times all told. The therapy was judged to be successful and was of 100 hr. duration. The most interesting finding was that the pretherapy total score indicated a "50-50 chance" for therapeutic benefit and was followed by a score obtained after but 8 hr. in therapy which placed the patient in the score range, "a little better than 50-50 chance." This score was very close to the final score obtained after 99 hr. of therapy. The authors suggest that since "success is not an abstract quality but is dependent upon the close relationship between a therapist and his patient, a record reflecting the potentials of the patient *in relation to the therapist* is a more realistic approach to prediction." The reviewers agree that an early therapy record might contribute more to prediction than a pretherapy record but would suggest that such a record might, at present, be more profitably viewed as a control on therapist-patient interaction, a variable which needs more attention than it has received.

Gibby *et al.* (44) compared Rorschach responses of three groups of patients: those who had been in psychotherapy six months or more, those who had 5 hr. or less of therapy, and those who refused therapy. The therapy was "psychoanalytically oriented." There were 33 patients in each group. Twenty significant differences were found between the first group and the other two groups as against five such differences between the second and third groups. The authors believe that their positive results, in contradiction to the negative results of other studies, stem from using extreme groups and controlling the following factors: sex, source of referral, disorientation, discontinuation as a result of extratherapy factors, and skill of the therapist. Although it is clear that there are differences between the groups, it is doubtful that all of the differentiating scores independently discriminate between the groups, for some scores are experimentally dependent upon the total number of responses.

Roberts (123) reported on "eleven Rorschach indices alleged to have prognostic significance for the outcome of treatment." All of the indices, when tested against three scales of improvement in psychotherapy failed "to meet the level of confidence established as the significant region for rejection of the null hypothesis." The author felt that only the color responses merited further investigation.

The results of the response-to-therapy studies of the Rorschach are very striking and may be summarized as follows: (a) Clinicians cannot predict response of the patient to psychotherapy from inspection of the scoring records. (b) Several studies report that response cannot be predicted from Rorschach signs and determinants. However, the study by Gibby *et al.* (44),

which employed what seem to the reviewers to be adequate controls, did establish differences between groups. Most important here was the intra-therapist control which at least partially accounted for such factors as skill, attitude toward the patient, etc. Their study also is characterized by the use of extreme groups. (c) The Rorschach Prognostic Rating Scale appears to predict response to psychotherapy with a rather remarkable degree of accuracy. (d) The criteria of these studies, instead of being conceptual diagnostic categories, are derived from behaviors which are rooted deeply in the personality structure and which occur in a socially significant interaction situation.

Why does the relatively simple and seemingly somewhat arbitrary Prognostic Rating Scale predict? In our opinion the reasons are as follows: (a) Scores are added in such a way as to increase reliability; more responses enter into total scores than is the case for single scores. (b) Scoring assigns fixed weights to specific types of responses rather than requiring the clinician to estimate weights for each patient in the context of all other responses.

The reviewers find these Prognostic Rating Scale results surprising for two reasons: first, Prognostic Rating Scale relationships are in general considerably higher than those reported in many other Rorschach studies; secondly, they compare quite favorably with the best results established in psychometric testing. In view of the importance of the results reported, these studies should be subjected to cross-validation in which such factors as level of initial adjustment (which may be related to final status), therapist skill, etc., are controlled. Some of the data suggest that the Rorschach Prognostic Rating Scale measures adjustment level which in turn suggests that the constructs underlying the scale might be carefully rescrutinized.

Although the studies reviewed above do in some cases reveal that the instruments used have predictive capacity, the question is whether the experimental variable, psychotherapy, was actually related to the predictive instruments. Admittedly it is difficult to obtain adequate control groups but the use of "own-controls," in which patients are tested before a "wait" period often necessitated by limited facilities and immediately before psychotherapy, is very often an entirely feasible method of obtaining information on the relation between therapy, the pretherapy assessments, and the criterion. The mere fact of prediction does not settle the question of whether what is ostensibly being assessed is actually what is being assessed.

Defined maladjusted groups.—In line with the tendency to make systematic appraisals of the clustering of symptoms, O'Connor (115) obtained 120 symptoms from the records of 300 male, psychoneurotic veterans. A cluster analysis of the intercorrelations of the 67 most frequent symptoms isolated 39 which were then factor analyzed to oblique structure. He found 8 factors, three of which were not recognized in clinical practice. O'Connor suggests that factor analysis in this context represents a relatively systematized approach to clinical classification. In this the writers concur. Since eight factors

were required to account for 39 symptoms, each symptom was loaded on two or more factors. Purely clinical techniques are likely to be too coarse to catch such subtleties. It is interesting to note that Mosier (113) obtained 8 factors on 39 items from a neurotic inventory using a college population. Several of his factors were similar to O'Connor's.

Lorr *et al.* (94) factor analyzed ratings of 73 symptoms, complaints, behaviors, and inferred motivations of veterans receiving psychotherapy in outpatient clinics. The ratings were made by therapists. They found 8 identifiable factors. Three factors were not related to conventional diagnoses although clearly recognizable clinically. The authors conclude that many common psychiatric syndromes may be identified by factorial methods. They might also have concluded that the factorial methods identify syndromes not identified in clinical practice. The studies of Beck and his associates (10), it will be recalled, indicated both agreement with psychiatric classification and the tentative identification of new classes of schizophrenia.

Guertin & Zilaitis (55) using MMPI items ran an inverted factor analysis on 24 patients diagnosed as paranoid schizophrenics. The twenty-fifth "subject" was a hypothetical normal individual whose scores were derived from normative data on the MMPI. Four distinguishable types were identified: the social normal (the hypothetical normal had a high loading on this factor), the grandiose and delusional, the evasive well-integrated, and the sensitive, inadequate, and withdrawn. In the opinion of the reviewers, high correlations between the factors indicated that a general factor of paranoid schizophrenia was present.

All of these factorial studies raise once more the problem of the multidimensionality of criteria and of stratification variables. It is quite possible that in many studies using diagnostic categories better results would have been obtained if the categories had not been treated as though each were homogeneous.

Stormont & Finney (148) studied two matched groups of 23 patients classified as violent and nonviolent. The groups were matched for age, IQ, length of hospitalization, and diagnosis. They compared the groups on the scoring categories of Klopfer, prediction of criterion from the scoring records, and on content of the Rorschach quantified according to a scheme devised by the authors. They found no differences between the two groups on location or content categories (with the exception of animal responses); absolute number of color responses; M: Sum C ratio, or movement responses. The violent group gave a significantly greater number of color-minus responses. The judgments of clinical workers showed only a chance correspondence with the categories violent and nonviolent, a result congruent with those of response to therapy studies noted earlier. However, the quantified aggression scores yielded a biserial correlation of .71 with the criterion, and use of a cut-off point on the distribution of the aggression scores led to 17 of 23 nonviolent and 19 of 23 violent patients being correctly classified. The authors conclude

that the quantitative approach to projective materials is a fruitful one. Here again, the criterion is socially significant behavior rather than some abstractly conceived category.

Shereshevski-Shere *et al.* (137) compared male alcoholics, normals, and schizophrenics with respect to anatomy content and $F+\%$ on the Rorschach. Patients were matched on age and total number of responses. They report that the anatomy response of schizophrenics outnumbered those of alcoholics whose anatomy responses in turn outnumbered those of normals. The proportion of alcoholics with $F+\%$ below 65 per cent was significantly greater than that of the normal group. Hampton (28) developed a scale for differentiating alcoholics which has the advantage that the responses are not obviously related to the purpose of the scale. Herr & Kobler (58) attempted to differentiate between 20 normals and 20 neurotics through their psychogalvanometric responses to 16 emotionally toned words. They found no significant mean difference but they did find a significant difference in variance between the two groups. It was not clear just how emotionally toned the words actually were for the subjects used.

Docter & Winder (27) reported on a comparison of delinquents and non-delinquents matched for age, mental ability, race, and socioeconomic level on the Porteus qualitative method for scoring the Porteus Maze Test. The qualitative scoring differentiated the two groups as shown by a critical ratio in excess of five. Taking the established cut-off score on the qualitative scoring, they found 70 per cent of the delinquent group and but 30 per cent of the nondelinquent group fell below the cut-off score. The authors suggest that the score is related to ego-control. A series of papers edited by Hathaway & Monachesi (62) contains several studies comparing delinquents with nondelinquents on the MMPI. The outstanding finding is that the Psychopathic Deviate and Hypomania scales "have an excitatory role in the actuarial numbers predicting the development of asocial behavior" (62, p. 136). The results on these scales led the editors to conclude that asocial, amoral psychopaths and the hypomanic are among the adult adjustment patterns chiefly presented by adolescent delinquents. Neurotic adult patterns appeared to be inhibitors of delinquency. Deri (26) also studied delinquents using the Szondi.

Kobler & Stiel (85) used the Rorschach to study patients diagnosed as involutional psychotics, as paranoid, and as depressed. The quantitative findings revealed that there were definite differences between the involutionals and the paranoid and depressed patients. However, only the qualitative analysis differentiated between the depressed and paranoid subgroups of the involutional psychotics. Dörken (28) in comparing tests on normals and psychotics found that the normals had higher intertest correlations on comparable variables and lower intratest correlations on discrete variables. These findings, if confirmed by subsequent studies, have important implications for assessment theory and practice because they imply, for example, different factorial structures for the two groups.

Normals.—Knoell (84) reported on the relation between teaching success and nine fluency factors. Ideational fluency was the only measure correlating significantly with a general rating of teacher success. She inferred from her data that good teaching is more closely related to facility in the expression of ideas than to quantity in writing fluency. Gough (53) constructed a personality test to predict academic grades. The mean r between the test and grades was .38 for 11 college samples; .36 for 8 high school samples. In 8 samples the mean r with measures of intelligence was .26; with measures of achievement .38. Stone (146) reported that differential prediction of achievement in various college curricula could be achieved by additions to a basic prediction battery of high school grade point average and the ACE Psychological Examination. The criterion was best predicted for students of commerce and elementary education by the basic battery; for physical science, the best prediction was obtained by adding the subtest scores for general science and literature on the Cooperative General Culture Test; for social sciences the best predictions resulted from adding the general science subtest. Multiple correlations ran from .51 to .73 with the criterion of grade point average in the basic curricula. Anderson (3) introduced two easy intelligence tests into a battery designed to predict grade point averages of college freshmen. The easy practice tests showed some predictive values higher than those of several more widely used tests such as the ACE Psychological Examination and the Cooperative English Test C2. Layton (88) attempted to predict success in a dental school using five tests in a battery of the Council of Dental Education. He found low relationships between the tests and honor point ratios in course areas where higher relationships might have been expected. He concluded that findings based on nationwide testing should be used with caution by any one school.

Long & Perry (93) using a weighted grade point average based on four years of college work as a criterion found that selection of freshmen engineering students can be improved by using high school averages and test scores. They also found that interest tests did not contribute to prediction but concluded that such instruments were useful for individual counseling. Rust & Ryan (128) reported that in general Rorschach variables did not differentiate between under- and over-achievers in college. However, the over-achievers gave significantly more P and A responses. The authors link this to superego functioning.

Hollander & Bair (69) investigated attitude toward authority figures in cadets who had successfully completed naval aviation training and in those who voluntarily withdrew from training. The groups were differentiable in terms of their attitudes toward their instructors. In a later study (68) on cadets just entering training, essentially the same results were obtained.

In the area of industrial selection the Empathy Test is still receiving some attention. Speroff (143) found that steel workers with high interpersonal desirability scores and low accident rates and those with low interpersonal desirability scores and high accident rates were differentiable on the test.

However, Van Zelst (157) reports that ratings of interpersonal desirability correlated .59 with scores on the Empathy Test. Hence it seems that Speroff was to a fairly great extent measuring the amount of correlation between the test and ratings of interpersonal desirability. Brown & Ghiselli (14) administered tests and an interest inventory to taxicab drivers using sales as a criterion. The zero-order correlations were low; a combination of the measures yielded a greater correlation.

Kriedt & Gadel (87) employed biographical data, interest, ability, and clerical aptitude tests in attempting to predict turnover among clerical workers. They found a multiple point biserial correlation with 3 month turnover of .40; with 12 month turnover, .33. Taking the lowest 10 per cent of the 3 month turnover group, they found that they had identified 36 per cent of the total group leaving in 3 months and had misidentified 4 per cent of those staying over 3 months. Meyer & Pressel (109) studied the relation of personality test scores to placement in the management hierarchy and found poor adjustment and emotional dominance to have valid management "hierarchy trends." Scores decreased successively at each level of the hierarchy, and social dominance increased. Herzberg (65) studied personality test scores of three groups and concluded that in industrial testing where faking is expected to occur, educational level is positively related to faking. Jenkins (76) attempted to predict the success of weather forecasting using interest schedules, aptitude tests, and general ability tests. The criterion of accuracy of forecasting correlated .31 with the names section of the clerical aptitude test. When used in profile relationship with the numbers section and a paper form board test, a cutting score on the clerical test eliminated 35 per cent of the cases in the upper half.

Handyside & Duncan (59) reported a study in the selection of supervisors in which candidates were screened by the normal company procedure and by means of an experimental procedure. Two criteria were used: first, ratings by managers, and secondly, number of promotions in a 4½ year period. They found that three experimental procedures were superior to the normal methods of selection. They concluded that a combination of existing elementary selection techniques permitted prediction of supervisory success as well as the uncertainty of the criterion allowed. Bruce (16) attempted to predict effectiveness as a factory foreman using 39 predictor variables. It was found that five predictor variables contributed to the obtained multiple correlation of .398 and that the major contribution of .290 to the correlation was made by a verbal intelligence test. The author concludes that further research in the area should be directed toward finding nonintellectual measures of the criterion.

Rock & Hay (124) investigated the use of test as predictors in a job evaluation situation. They found that the TAT² and sociometric questionnaires, after preselection by other tests and data, can differentiate between the leaders and the members of a committee. They also found that the com-

mittee could accomplish more work under the predicted leader than when a member functioned as the leader.

TEST-ORIENTED STUDIES

Rorschach.—This technique continues to be the topic of many publications. Several new manuals have appeared. Allen (1) has written an introductory text and Halpern (56) has provided one on the Rorschachs of children. An advanced guide is offered by Phillips & Smith (118). Their candid comment applies to all these works:

This book . . . is composed largely of statements about relationships between Rorschach performance and other behavior. Unfortunately, few of the relationships which are asserted have been corroborated; most represent guessed-at laws. (118, p. v)

They go on to assert that these relationships are open to empirical testing. With regard to interpretation, they imply that a scoring factor must always be interpreted in terms of the same attribute.

A more ambitious work is the first volume on *Developments in the Rorschach Technique* by Klopfer and others (83). It attempts to provide a manual to discuss research findings, and to relate Rorschach and personality, with uneven and partial success. Klopfer presents a chapter on his Rorschach Prognostic Rating Scale without supporting data on its development or validity. Ainsworth's chapter on "Problems of Validation" is the most pertinent one for our topic. While this author cites many studies, the value of her survey is greatly curtailed by what appears to us as marked prejudice. Favorable evidence is accepted uncritically and negative findings are dismissed summarily, or strangely interpreted: "It is perhaps indirect evidence of the validity of the Rorschach technique that it fails to establish clear-cut differentiation between diagnostic groupings while yet showing undeniable relationships to them" (83, p. 464). If her position on the central problem of the methodology of Rorschach validation is consistent, it is unclear to us.

The only book on research findings using the Rorschach is that of Beck (10) discussed above. Two papers discuss the content analysis of the Rorschach, both from an analytic viewpoint without empirical data. Brown (15) offers suggested interpretations for many responses, and Schafer (131) considers content in the light of the process of response.

The problem of developing useful norms is underlined by two reports. Wedemeyer (159) reports data on 136 young Navy enlisted men of average intelligence. She characterizes the records as meager and atypical. Neff & Glaser (114) tabulated statistics for 100 consecutive cases at a vocational guidance agency, broken down by normals, neurotics, and psychotics. Comparisons are made with other studies. Since Neff & Glaser's cases had a mean R (total number of responses) of 41, Wedemeyer's group a mean R of 14, and Beck's normal group a mean of 33, it is difficult to see how such norms can be applied usefully in practice because of the varying and complex relationships between R and the several scoring categories (37).

An extensive study of repeated Rorschach protocols from adolescents was made by Ives, Grant & Ranzoni (75). They provide useful data on relationships between scoring categories and age in the 11 to 18 period. Since well-known "signs" of neuroticism appeared too frequently and those of adjustment were too infrequent, they conclude that such signs are not useful in interpretation of adolescent protocols and need re-examination before use with adult records.

A thorough exploration of configural scoring of the Rorschach was made by Anderhalter (2). While only one score out of 40 differentiated two criterion groups, techniques such as Cronbach & Gleser's *D* (24) and the linear discriminant function were successful, especially on cross-validation.

Several studies have used experimental manipulations to improve our knowledge of the Rorschach. A new study of the effect of color compared the standard cards with an achromatic set: Brody (13) reports that the presence of color does not affect many of the scoring categories. However, neurotics who took the standard test before the achromatic one were less consistent from test to retest, an effect not found in the normal group or the neurotic group receiving the achromatic set first.

Other researchers varied the subjects' experiences preceding the testing. Calden & Cohen (18) found that Rorschach performance was related to instructions (task-oriented versus ego-involving), the examiner's definition of the test (as measuring intelligence, imagination, or nervousness), and the interaction of these factors. Training on the Street Gestalt Test and the instruction to give whole responses only on the Rorschach were shown by Keyes (81) to have the expected effects, especially for subjects exposed to both conditions. Hypnotically induced hostility toward the examiner affected the protocols, but did not increase white-space or color responses in Pattie's study (116). Exposure to anatomical pictures was found by Rabin, Nelson & Clark (120) to have no effect on protocols of males, but exposure to nudes and other "sexy" pictures did. Sex responses were not, however, so frequent when the examiner was female. [The unproductive records in the previously discussed report of Wedemeyer (159) might also have been attributable to the use of female examiners.]

Some progress is being made on testing Rorschach hypotheses against non-Rorschach behavior. For a schizophrenic group, Singer & Spohn (138) found that a large number of movement responses and introversive experience-type were associated with a low level of gross motor activity (in a waiting room) and with motor inhibition as indicated by ability to write very slowly, the relationship being more pronounced for subjects giving active movement responses. The color sum index showed some positive relationship with gross motor activity. Linton (92) reports some Rorschach correlates for response to suggestion in the autokinetic situation. However, many of his variables are related to total number of responses (37), and this variable was not examined or controlled. White-space responses were moderately related to teachers' ratings on negativism but not to assertiveness, inade-

quacy, or self-distrust in Bandura's research (6), the relationship being comparable for both extratensives and introversives. Bills (11) found several Rorschach signs related to depression (as measured by self-ideal discrepancy). Grant *et al.* (54) report insignificant but slightly positive correlations between adjustment as rated by Rorschach judges and adjustment as rated by caseworkers. Both sets of ratings had reasonable interjudge agreement. Ratings based on the whole protocol were not systematically more related to the criterion than those based only on the formal scoring categories.

Two studies test the proposition that the Rorschach can be used to predict response to stress, both using decrement of performance on the Digit-Symbol Test as the index. Stopol (147) concludes that his 12 specific hypotheses are not confirmed for either failure stress or task-induced stress. Similarly, no substantiation for previously reported Rorschach indices of stress-induced decrements was found by Westrope (161). However, extreme groups on the Taylor Anxiety Scale were discriminated both by global judgments and by some specific indices.

The relation between the Rorschach and various forms of aggressive behavior was studied by Ingram (73). Subjects high on white-space responses showed more hostility and initiative, but also more co-operation and rapport and less resistance in a frustrating interview situation. In a puzzle situation with minimum social interaction, they did not show more aggression, but rather less assertion and hostility. Judges' ratings from the Rorschach were not related to ratings based on either of the situations [cf. Stormont & Finney (148), discussed above].

In summary, the healthy trend toward empirical studies of Rorschach performance has continued. It would appear that behavioral correlates for some scoring categories may become firmly established even though many reported indices fail to stand the test of cross-validation. Information is accumulating about the subject's response pattern as a function of his interaction with the total testing situation [cf. Miller (110)]. However, the technique seems to permit such freedom of response that maximum utilization of the extensive information contained in the protocol as a whole seems feasible only when mediated by an interpreter explicitly using a subjective metric, e.g., Beck (10).

TAT.—The literature on the TAT contrasts sharply with the papers on the Rorschach. It is less extensive and seems concerned with the TAT as a technique rather than as a diagnostic aid. Only two or three studies compare TAT protocols with other behavior. McClelland *et al.* (98) have brought together in a book their work on a five year project on the need for achievement. Their purpose was to develop a method of measuring human motives and to use it to collect data contributing to a theory of motivation. This is an excellent example of careful experimental work on a projective test in conjunction with external behavioral indices. McClelland's theory of projection has been questioned by McArthur (97) whose findings suggest self-projection alone can account for TAT content. Some predicted motivational

differences between males and females were verified by Lindzey & Goldberg (91). Definite but not high similarity between TAT stories and dream content is reported by Gordon (50). Studies by Armstrong (5) and Light (90) indicate that animal pictures (as in the CAT) are not better than human pictures for use with children.

Other projective tests.—A fruitful analysis of the process involved in Sentence Completion responses has been made by Hanfmann & Getzels (60). From an inquiry, they conclude that a preponderance of the material is at or near the conscious level. A similar technique was applied in a study of the authoritarian personality, using this test (29). Low positive correlations between adjustment indices from the Incomplete Sentences and other sources are reported by Rotter *et al.* (126).

An English translation of Szondi's book is now available (149). David (25) has compiled a 332 item bibliography on the Szondi test. The additivity assumption in this test is rendered questionable by the findings of Cohen & Feigenbaum (22). Furthermore, the degree of association among the several pictures in each set differs from factor to factor. In studies by Fleishman the discriminative power of Szondi's syndromes is questioned (39), and "the quotient of tendency tension did not manifest its imputed discriminative function" (38, p. 46). Gordon's factor analysis (51) of the pictures brings out again that liking is related to the portrayed patient's age, sex, mood, etc.

Among the papers on drawing techniques, a valuable one is Sloan's critique (139) of validation studies of the H-T-P (house-tree-person), which emphasizes the lack of sound research design. Whitmyre (162) found that adjustment as rated from drawings of the human figure did not discriminate normals from psychiatric patients. Adjustment ratings were highly correlated with artistic excellence. An extensive study of the DAP (draw a person) technique by Blum produced no consistent agreement between ratings from the drawings and those made by the psychiatrists and others. His dubious conclusion is, "The Machover DAP technique has highly questionable validity, but proves to be no worse than any of the other common clinical personality assessment procedures" (12, p. 125).

In an intensive study of the Bender-Gestalt, Gobetz (49) failed to support two manuals for the interpretation of this test but offered a set of "neurotic" signs which he cross-validated.

Paper-and-pencil tests.—Normative data on the MMPI have been published (160); two studies (4, 154) point out differences between their groups and the standardization group. Low correlations (.16 to .39) are reported by Schofield (134) for retest after two years of medical school. The value of K corrections was questioned by the findings of Tyler & Michaelis (155). Data on F minus K are offered (103). The value of the MMPI is defended by Calvin & McConnell (19).

The stability of Kuder Preference Record scores was reported by Rosenberg (125) to be lower for the high school age than that found by Herzberg

et al. (66) for a comparable time interval from high school to college or work. Herzberg & Russell (67) give findings supporting the validity of this test, but Iscoe & Lucier (74) present correlations which indicate that the scales of the Kuder and Allport-Vernon are largely independent of each other.

Other studies.—A number of validation studies on other (usually less common) instruments have been reported (48, 72, 101, 121, 144, 157). New techniques have been developed (20, 21, 40, 130) and adaptations of old ones have been explored (63, 95, 100, 105, 112, 152, 153). Ebel (31) has studied item response times.

Marks (106) criticizes the diagnostic value of the Wechsler-Bellevue on the grounds of low efficiency, validity, and univocality. The order of items within subtests is questioned by Mech (108). High correlations between the Colored Progressive Matrices and the Wechsler Scale for Children have been reported (107).

Of four contrasting papers on the interview, two review research findings (23, 79) and two do not (122, 129).

Falk & Bayroff (35) demonstrated that criterion ratings may be contaminated by the form in which they are collected. Intercorrelations among the characteristics of personality items are examined by Gordon (52).

A report of Witkin's extended research program on *Personality Through Perception* (163) should be mentioned. While this book reports many high correlations between perceptual variables and scores from projective tests and interviews, the reader must interpret these data with caution: the "interview ratings" were based on the interview plus other data; many of the other correlations involve variables established empirically, not by a priori postulation.

Using a "fractional omission" design, Kostlan (86) found that inferences based in part on the Social Case History were more accurate than those based on minimal identifying data. The presence or absence of the Rorschach or the Sentence Completion had no apparent effect on accuracy scores.

In a valuable analysis of the processes in projective testing, Secord (136) suggested that the structure of a test situation be defined as the homogeneity of subjects' definitions of the test situation. He points out two alternatives: personality tests must be maximally structured (as in psychometric testing of achievement), or adequate means for determining the subject's definition of the situation must be developed. However, even if the subject's definition could be classified with confidence, such a procedure would greatly complicate assessment work because of the difficulty of comparing the responses of subjects who defined the test differently.

ASSESSOR-ORIENTED STUDIES

Increasing attention is being paid to the contribution of the assessor to the assessment process. A systematic analysis by Miller (110) points out that both the examiner and his relationship with the subject must be considered in Rorschach interpretation. Van Krevelen (156) demonstrates that

the presence of an examiner affects Szondi protocols. Gibby *et al.* (46) show that significant differences exist among the Rorschach profiles obtained even by experienced examiners, the inquiry being the locus of this effect (45). The higher an officer is rated (on aptitude, etc.), the higher his rating of others correlated with the mean ratings of peers (132). A very high relationship was found by Hammer & Piotrowski (57) between the rated hostility of the clinician and the amount of hostility he inferred from drawing-test protocols.

The importance of the assessor's frame of reference was demonstrated by Soskin (142). In two other studies, he showed that predictions based on projective protocols have a bias toward maladjustment. He also reported that such predictions made by experienced clinicians were not significantly more accurate than those made by novices (140, 141). Grant *et al.* (54) report a bias toward maladjustment in ratings from the Rorschach. Dymond (30) found that therapists' predictions of their clients' self-descriptions at the end of client-centered therapy are generally (but not always) more accurate than "predictions" based on the therapists' sorts for the average client. "Intuitive" predictions were reported by Hovey & Stauffacher (71) to be more accurate than "objective" predictions. The interpretation of this finding, which is contrary to that of many previous studies, is hampered by the paucity of data in their publication. Many previous studies, including some current ones (73, 148, 161), do not find a difference in favor of intuitive predictions.

CONCLUSIONS

Consideration of both the studies reviewed in this chapter and the even larger number examined but not included here has left the reviewers with these impressions:

(a) With very few exceptions the studies pertinent to assessment are restricted to particular criteria, specific instruments, and even to separate derived scores. As a result few general principles have emerged, have been clarified, or have been evaluated although some excellent studies have been reported.

(b) The trend toward the testing of clinical hypotheses, appearing in the postwar years, has become stronger.

(c) Recent developments in the theory of experimental design, in distribution-free statistical methods, and in psychometric methods are not being exploited. Important controls have been omitted in many studies. In others it is difficult to ascertain the controls employed. Furthermore, the rather frequent omission of critical data and tables often makes evaluation of studies difficult.

(d) Insufficient systematic work on criterion behavior continues to vitiate much research. On the other hand the psychometric properties of many tests, especially free-response tests, are far from being fully understood.

(e) Techniques such as configural scoring, and possibly latent structure

analysis, should soon allow the systematic analysis of the vast quantity of information, recognized and unrecognized, contained in the responses to a single assessment test.

(f) A few, but far too few, investigations have been concerned with the really fundamental problem of the subject's perception and definition of the testing situation.

(g) The potentialities of subjective metrics for progress in the personality domain should permit developments in directions not possible under the constrictions imposed by classical psychometric methods which center around the concept of difficulty.

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PSYCHOTHERAPY¹

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The publications examined in preparing this review include all issues of some 40 professional journals in English from the fields of psychology, medicine, and social work appearing from May, 1953, through April, 1954, inclusive, as well as relevant books published during that period. The articles cited constitute perhaps one-third of those read.

I have employed a fairly restricted meaning of the term "therapy," although I have not confined myself to work done in medical settings or utilizing subjects with psychiatric diagnoses. Studies of "personal counseling," marriage counseling, or social casework have also been included if they dealt with measures, processes, or methodological questions pertinent to psychotherapy. Papers specific to educational, industrial, or vocational counseling and guidance were not covered. I have excluded papers dealing with drugs, surgery, and other strictly medical adjuvants to psychotherapy. Material primarily concerned with legal, professional, or public-relations problems (e.g., privileged communication legislation) is not included. I had originally intended to cite mainly experimental or statistical studies to the relative exclusion of purely anecdotal and impressionistic material, but this policy would yield such a thin chapter that it was abandoned. I have, therefore, included some "studies" which lack controls or which employ highly contaminated criteria.

Impressionistic reports of clinical experience or presentation of selected case material have ordinarily been cited only if they present some novel idea or technique, or call some received doctrine into question. Articles presenting familiar evaluative claims for a recognized procedure but offering no evidence, such as "We use group therapy in our jail and we like it fine," have been generally ignored. Regardless of their theoretical interest, for reasons of space I have not been able to include papers mainly emphasizing the psychodynamics of a single case which were elucidated in therapy but were not related in the article to therapeutic technique or outcome (such as commonly appear in psychoanalytic journals).

INDIVIDUAL THERAPY

Outcome.—The minimum standards for an adequate outcome study obviously include (a) a control group, (b) pre- and posttherapy evaluation procedures which are either "objective" or, if judgmental, are uncontaminated, (c) follow-up of both groups, preferably repeated so that exacerbation

¹ The following abbreviations have been used in this review: DRQ (Discomfort Relief Quotient); MMPI (Minnesota Multiphasic Personality Inventory); VA (Veterans' Administration).

and remission rates can be estimated and the curves extrapolated. I am saddened to report that perusal of over 200 journal articles and a dozen books reveal one paper approximating these desiderata. (Relaxing requirements by forgetting follow-up but insisting upon controls does not change this figure.)

Dymond (19) constructed an "adjustment score" from self-sort placements of the 100 *Q*-sort items in use in the Chicago Counseling Center research project, scoring an item as positive or negative on the basis of judgments by two non-Rogersian clinicians. Clients seeking therapy showed no significant change in score during a two-month waiting period ($N=11$). Her tables indicate this was not merely attributable to the N being too small to prove a real shift. After therapy there was a significant improvement; the treated cases ($N=25$) were no longer significantly different from the normal controls ($N=15$), from whom they were markedly different before treatment. A six-month follow-up repeat on 22 treated cases showed a mean almost identical with that immediately posttherapy. One's evaluation of these findings hinges upon his view as to (a) psychological significance of a self-sort on face-valid items, (b) whether the psychological situation of a "waiting-for-therapy" client permits such cases to be considered an adequate control group.

Several additional studies have some value because they use relatively objective criteria of change, but they lack either controls or follow-up or both. Seitz (81) presents the results of "dynamically-oriented brief psychotherapy" with 25 cases of excoriation syndromes. A 12-session series was predetermined for each patient. The characteristic psychodynamics of such cases were formulated in advance, on the basis of previous studies. The therapist directed the topics covered in each session, aiming at interpretation of the more superficial mechanisms. Twelve patients discontinued treatment unimproved. Of the 13 who followed through, 12 achieved a symptomatic cure ("skin clear"). These remained clear at three months follow-up, and of the 7 available at six months follow-up 6 were still clear. A one-year follow-up of 5 of these cases showed one additional relapse. During the treatment course all 25 cases showed temporary aggravation of the cutaneous condition. Among the symptomatic cures follow-up at three months showed "the original masochistic ego defenses were again assuming dominance; and after a year there was little evidence to indicate that the treatment had provided a corrective emotional experience of any permanence" (p. 210). Three of the 12 cases of symptomatic cure were also judged as "failures" because they developed serious acting-out in extra-therapeutic situations. The author suggests that the highly structured interview method, together with its active provoking of hostility but avoidance of detailed working through of resistances, may have been responsible for the failure rate. Summary tables of patient characteristics and brief abstracts of all the interviews are presented.

Gallagher (31) studied MMPI¹ changes of 41 college students treated by

"client-centered" methods (median of five to six sessions). Significant declines occurred in six scores, the largest on Depression (about seven *T*-score points). "Feeling of discomfort" scales showed changes while the more "character or behavior disorder" scales did not. No control cases were available; therefore, in the light of evidence that MMPI shifts to the normal on retest and of the usual regression problem, the study would not be readily interpretable. However, a correlation of .44 was obtained between (a) a verbal measure of the positive-negative feelings expressed in the first and last interview and (b) a total maladjustment change score on MMPI. It is interesting that the therapist rating did not correlate with the latter. Gallagher also (32) studied changes in four scores or indexes obtainable from MMPI in this same group of clients. The Taylor Anxiety Scale (truncated), Winne Neuroticism, Welsh Anxiety Index, and Welsh Internalization Ratio all showed significant decline with therapy. Changes were not of great magnitude, running around a half-sigma on his distributions. He points out that his correlations between these test verbal measures and client self-rating, therapist rating, or the positive-negative statement ratios in the protocols, are of ambiguous value, since they all involve various kinds of self-evaluating talk by the client or inferences therefrom.

A five-year follow-up study of the results of social casework was offered by Kogan, Hunt & Bartelme (50). The follow-up interviewer was a highly experienced clinical psychologist and was uncontaminated by knowledge of the previous casework contacts (p. 6). After listening to the client's account of his casework experience the interviewer reflected prescaled evaluative statements for the client's acceptance. "Free statements" were sorted out-of-context on an 11-step scale by five independent judges, and a pooled free-statement score correlated .85 with the single reflected evaluation scores. No relationship was found between the *DRQ*¹ shift during casework and the client evaluations of help received, nor between independent caseworker ratings on "movement" and the client evaluations. These negative findings are not attributable to low reliability (p. 10). There was no correlation between movement during case work and movement subsequent to closing. The best single predictor of closing status of adjustment was opening status ($r = .65$). Experienced caseworkers were able to predict follow-up client evaluations somewhat from a reading of the case material ($r = .52$; p. 17).

I think the most interesting finding of the foregoing study is a methodological one. Three reliable measures (client evaluation, case-reader movement rating, and *DRQ*¹ shifts) are negligibly correlated. Such a fact should make us cautious about single "criteria" used in the evaluation of therapeutic outcome. As to the substantive question, "Does casework have a persisting effect?" the authors conclude that without controls this question is unanswerable. Their data at least indicate that the clients did not tend to regress over the five posttreatment years. The authors suggest that the follow-up period itself might be taken as a base for estimating "movement" unaided by treatment. They discuss the objections to such a base (pp. 61-66)

but do not make very explicit the most serious flaw, that when people enter casework they are badly off, and there is no reason for expecting them to "keep getting better and better" once they are relatively nearer the norm. The authors are not really seduced by their own suggestion but wisely close the book with, "To yield definitive evaluative evidence, the follow-up method should be combined with some form of control group design" (p. 103).

Wolpe (91) argues that psychoanalysis is a theoretically unsound treatment and its practical results are actually poor. He thinks that the main common factor is reciprocal inhibition of neurotic anxiety responses (p. 825) [cf. Shoben (82)], and that therapists may accomplish this while "aiming at something quite different." The obvious course is to institute therapeutic regimes which are directed specifically to that end, instead of relying upon more traditional methods which are based upon some other theory and which achieve the reconditioning goal only as a (possible) by-product. Wolpe, therefore, proceeds by giving his patients "behavior prescriptions" (p. 826) as to their behavior in certain real-life situations, particularly those ordinarily involving suppression of hostile reactions. He denies that the patient is asked to "put on an act" (p. 826), but does not furnish concrete examples of his own interview tactics; thus one is left rather in the dark as to how the "tasks" are set. He does indicate a similarity between his technique and the methods described earlier by Salter (75, 76) and Herzberg (44).

In cases of anxieties elicited by inanimate objects, Wolpe uses systematic training in conditioned relaxation and states that "powerful autonomic effects" can be observed in a well-trained patient who has learned to relax local musculature rapidly. Other adjuncts utilized in the therapeutic session are progressive hypnotic desensitization, role-playing, and "abreaction." He presents statistics on 70 cases treated by this method, seen from 4 to 125 interviews. Nine of them had been previously psychoanalyzed "with little or no effect" (p. 827). He classifies the cases into four levels of outcome, based upon Knight's five criteria (48) plus changes in the Thurstone-Willoughby neuroticism score. Apparently, he himself made the evaluations, and no control cases or follow-up data were available. No actual score data on the personality test are presented. He reports his results as 49 per cent apparently cured, 37 per cent much improved, 10 per cent slightly to moderately improved, 4 per cent unimproved. Of the nine previously analyzed cases, two he considered "apparently cured" by his methods and five "much improved" (p. 827). In spite of an illegitimate use of chi-square, it is a provocative article. I predict it will be largely ignored, since it espouses a position out of phase with the *Zeitgeist*.

Although based on only three cases, Gottschalk's report (39) of analytic therapy in epilepsy is quite impressive because of his graphs showing dramatic changes in seizure frequency maintained over a long period. Beckham (7) in an inadequately reported study claims a 12-point IQ rise in counseled students which was not found in uncounseled "controls."

Considering the extreme shortage of therapeutic personnel, optimal utilization of professional time is of great importance. The scarcity of good empirical studies of selection and prediction is discouraging. Barron (5) reported a study of prognosis from psychometrics which, in spite of its small *N*, could serve as a model for such investigations. Thirty-three adult, lower-middle-class psychoneurotics received outpatient psychotherapy, one session a week for six months. Two experts, on the basis of careful study of all the nontest data and discussion with the therapist, made independent judgments of the therapeutic progress, showing high inter-judge reliability (.91 on a 100-point scale of "Improvement," and 31 out of 33 agreements on a dichotomous classification). An abbreviated Wechsler IQ correlated .46 with improvement. The MMPI profile, as sorted by skilled users, yielded 62 per cent correct classifications, while two forms of a "mechanical" profile rule for MMPI decision gave accuracies of 75 per cent and 80 per cent. The Rorschach had no predictive validity, using either the traditional determinants and ratios, the Harris-Christiansen prognostic index, or the judgments of four skilled Rorschachers based on the entire protocol. The California Ethnocentrism scale turned up as the best single predictor, correlating $-.64$ with "Improvement" ratings.

Barron also derived a 68-item MMPI key, based upon his carefully studied sample, as an aid in predicting response to psychotherapy (6). It had an odd-even reliability ($N=126$) of .76 and a three-months test-retest reliability ($N=30$) of .72 on clinic patients. Three cross-validation samples of variable constitution and criterion fallibility yielded correlations of .42, .54, and .38 with improvement. On the basis of item-content, correlation with other MMPI variables, and several additional relationships he considers the key to be a measure of ego-strength.

Schaffer & Meyers (77) studied the relationship between the social class of a psychiatric patient and what subsequently happens to him in an outpatient clinic. They analyzed the contact records for all patients applying for therapy during one year on whom the Hollingshead "index of social position" (45) was determinable ($N=183$). Patients at this hospital are screened out if they can afford private care, and fees are scaled according to patients' means from \$0.50 to \$5.00 per visit. Decision as to whether to undertake formal therapy and the assignment of patients to therapists was a matter of group consensus in an intake conference. Residents and others were largely free to choose and reject patients. The assigned therapist enjoyed considerable autonomy in the matter of duration and intensity of contacts. Supervision was available (psychoanalysts or analytically trained psychiatrists) but "seldom . . . imposed" (p. 85).

In this study there emerges a remarkable contingency between the patient's social class and decisions of the intake conference ($P < .001$). We find, for example, that 64.7 per cent of patients from the professional-and-executive class were assigned for therapy to senior psychiatric staff and residents, whereas only 2.4 per cent of the lowest class and only 33.4 per cent

of the next-to-lowest class received such high-echelon therapists. None of the top class and only 9.6 per cent of the second-level class received their psychotherapy from medical students, while this was the treatment given to 26.4 per cent of Level IV patients and 23.8 per cent of Level V patients. A patient of bottom-class status has between five and seven times the likelihood of being "not recommended for therapy" as does the patient from one of the top classes (Table 3, page 87). It was further shown that duration of clinic contact (in weeks) was intimately related to social class ($P < .01$). [For an analysis of a subgroup of these cases in terms of number of interviews, see Auld & Myers (3)]. Source of referral, age, sex, neurosis-versus-psychosis, profession of intake interviewer were all insignificantly related to clinic handling, although data on these factors are not presented in the article (p. 88).

The authors discuss several (not incompatible) hypotheses as to the causation of these impressive relationships, including: class differences in the conception of mental disorder and its treatment (e.g., ability of cultivated adults to "think psychologically" about illness); difficulties in communication between a therapist and a patient of very disparate class origins; implicit evaluations by therapists as to what sort of patient is really more "worthwhile"; the conception of the therapist, in some degree imposed by the clinic group, as to what sort of therapist one wants to be, and hence, what type of patient one wishes to work with in order to play that kind of approved therapist-role. Since the usual economic selective factors of private practice are not reflected within the group of patients treated, "it seems to be evident that an economic theory of the stratification of psychiatric practice does not have general validity, and we believe that the relevance of the economic component of social class in this connection is more limited than has been thought" (p. 93). This study has implications for the common training situation in which a student therapist is being controlled by a consultant whose own daily practice deals with a markedly different clientele. The type of therapy we train for and write about may be quite unsuitable for the majority of neurotic persons.

Rogers & Hammond (73) and Roberts (72) reported failure in the attempt to predict outcome of psychotherapy by various Rorschach signs. Gibby *et al.* (34) mistitle an article "Prediction of Duration of Therapy from the Rorschach Test" when no prediction was carried out, and their data do not indicate that prediction would succeed if tried. Pumpian-Mindlin (70) presents his opinions on the selection of patients for short term therapy based upon a systematic staff review of cases handled thus in a VA¹ Mental Hygiene Clinic. Presence of severe psychopathology seemed by itself no contraindicator to short term therapy. Movement in the initial interviews, power to "grasp and use the material," was the single most important factor. Other positive clinical criteria for selection include the therapist's implicit evaluation of the "worthwhileness" (p. 645) of the patient; fantasy-reality or aspiration-achievement concordance; frustration-tolerance; adequacy of past

and present object-relationships, especially his ability to view parents and spouse as human beings with good and bad qualities rather than "over-polarized" as good and bad figures; stability with ameliorability of present environmental situations. Mitchell, Preston & Mudd (59) showed that certain characteristics of the initial interview could predict features of the subsequent case development in a marriage counseling agency; but since the median number of total contacts was only three, such a finding is difficult to interpret.

Process.—Outcome research is not likely to lead to much improvement in specific interview tactics unless adequate methods can be devised for categorizing individual client and therapist responses in significant ways. Collier (13) constructed a Thurstone scale of 11 types of therapist responses, based upon the ratings of 26 psychotherapists as to how "resistance-penetrating," "uncovering," "interpretive" they considered each type of response to be. The response-types were discriminable, the lowest scale value being that for silence and the highest being for a response in which the therapist "abstracts common dynamic elements . . . from repetitive occurrences . . . and brings this relationship to the recognition of the patient . . ." (p. 322). Application of the scale to therapist responses in selected published protocols by therapists of different persuasions showed expected trends. Thus, not a single therapist response in Axline's *Play Therapy* (4) had a scale value above four, whereas three analytic protocols yielded responses over the entire range. Discriminability was also shown for 50 samples of verbal exchanges judged by a group of therapist readers. Several applications of the scale are suggested, among them a check on Fiedler's view (25, 26) with which Collier disagrees, that there are relatively minimal differences among experienced therapists of different schools.

Phillips & Agnew (69) presented hypothetical client responses to students and to clinicians at two training levels, the task being to select from one of five possible counselor responses the one they would make as counselor. Marked differences were demonstrated between "naive" and "trained" judges, the trained judges choosing evaluative, supportive, and interpretive responses much more rarely than the students did, while students selected many fewer reflective responses. The authors believe that this study argues against conclusions sometimes drawn from Fiedler's work, and they argue that (a) Fiedler's Q-pool consisted of "banalities" (p. 283) which would not be expected to discriminate experts from laity; (b) Fiedler's two naive subjects were far from being naive; (c) what one does in therapy betrays one's attitudes more than his verbal statements. They conclude that their study indicates an element of clinical skills or methods going beyond " . . . a simple extension of knowledge of interpersonal relations possessed by any reasonably intelligent and emotionally mature person" (p. 283).

DeMichele (16) used still photographs of persons showing varying levels of emotion to show that analysts infer more pathology than "eclectic" therapists, while Rogerians infer the least. The ratio of inferences made to

cues stated was highest for analysts and least for Rogerians. The general Rogerian avoidance of inferences renders the author's use of the term "depth" for this ratio somewhat misleading. Tucker (87) found no relationship between 43 clients' self-evaluations of benefits of nondirective therapy (median 6.5 sessions) and either counselor evaluations, evaluations of protocol readers, or an improvement index based upon shift in a "negative-to-positive" statement ratio from first to final interview, although these latter three indexes were reliable and correlated .62, .57, and .53 among themselves. Snyder thinks this indicates need for caution "in employing exclusively phenomenological measures of change" (83, p. 132). Gillespie (35) reports a lack of correlation between verbal signs of resistance and "success in therapy" as measured by a composite criterion of Tucker's four variables, although a majority of cases showed a decrease in resistance from first to last third of treatment. Gallagher (33) reports insignificant differences between "stayers" and "dropouts" on several Rorschach and MMPI measures, but stayers checked more problems on the Mooney Problem Check List. Several other negative findings are reported in the Snyder collection, which should be carefully studied by anyone contemplating research in psychotherapy.

High correlations between reliable judgments (from recorded sessions) of therapeutic success and several measures of degree of identification between patients and their therapists were reported by Schrier (80). *Q*-sorts by therapists of their own Murray need-variables were *Q*-correlated with the patient's need-profile, the latter obtained from a 300-item face-value questionnaire. One possible flaw in this interesting and careful study is the possibility that patient-therapist correlations may become high with successful cases because to be more "healthy" is to be more like therapists, since (one hopes) the latter tend to be healthy. One would like to know the correlation between each patient and all of the therapists in the experiment before calling this "identification."

Murray (61) plotted curves of incidence of statements of "hostility," "intellectual defense," and "physical complaint defense" in the protocol of a case treated by supportive-interpretive therapy (17 hr.). Replacement of one defensive style by another and rise in manifest hostility as defensiveness declines is shown and is not apparently attributable to mere internal numerical constraints. In both this and a second case, the therapists' major interpretations were followed by sharp decline in incidence of the defense interpreted. A learning theory formulation is offered.

Dollard, Auld & White (17) detail the treatment of a case by short-term uncovering therapy, with emphasis upon the process of supervision and the underlying theory of tactics used. Since this kind of thing is so rare among non-Rogerians, it is unfortunate that a better case was not chosen. The material presented does not give adequate support for the dynamic formulation employed, and it seems very doubtful that much of a therapeutic result was achieved. The appendix contains some singularly feeble use of psychometrics. Pumpian-Mindlin (70) points out that the

techniques of short term therapy are "extremely varied" but can be summed up as "accentuate the positive and eliminate the negative" (p. 647). The relatively great directiveness of the therapist is shown mainly by selectivity in what is picked up and by "skilful neglect" (p. 647) of productions not directly related to the presenting problems, rather than by greater interview participation. Transference is handled by "deflecting the specific problems being dealt with in therapy onto an important figure in the patient's environment rather than focusing the problem around the patient therapist relationship" (p. 649). Acting-out which is "good" is encouraged by allowing its acting-out features to go unanalyzed. Uncovering of genetic childhood conflicts is not stressed. In general, all interpretations are done in terms of object-relations rather than drives and impulses or the "internal world" (p. 650). It is important that both patient and therapist be genuinely willing to accept improvement or cure of the presenting problem as the therapeutic goal.

Train (86) offers a discussion of "flight into health" as a defensive tactic, and probably a source of some "apparent cures" which would be revealed as spurious by systematic follow-ups. "By and large, however, all writers are agreed that . . . it is a sign of therapeutic failure, since it ends treatment and provides through 'apparent' health, an escape from 'further unpleasant truths' " (p. 465). Whether he assumes that all or most spontaneous remissions "[becoming] asymptomatic without professional assistance" (p. 465) subsequently exacerbate is not entirely clear, but presumably not. If they do not, it is not obvious why analogous remissions assisted by some uncovering therapy should be expected to do so. He does suggest that this method of defense may have value for certain types of cases, one of which (three-year follow-up) is described. He suggests a program of treatment for appropriate cases in which an "iatrogenic, controlled, and planned 'Flight into Health' plays a role" (p. 482). Reider (71) discusses the phenomenon during analytic therapy in which the analyst presents interpretation in the form of a "construction" (30), with resulting symptom-aggravation and production of further screen memories. A case study is presented. Special resistance phenomena in the forms of evasive speech and a spontaneous quasi-hypnotic state are briefly discussed by Evans (22) and Fliess (28), respectively.

SPECIAL TECHNIQUES AND CASES

Boverman (11) in a sensible paper on the psychotherapy of delusional patients, points out the artificiality and indirectness often shown by therapists in "defining the reality-situation." He argues that treatment should begin by a kind but firm "confrontation" with those aspects of reality that are crucial to the patient's having an adequate definition of the relationship. If the therapist is circuitous about establishing such basic matters as that he is a physician, that the patient is ill, that the therapist knows that the patient has false ideas, and so forth, this may often block any attempt at forming a good therapeutic relationship. We routinely try to clarify certain

surface misunderstandings and misidentifications in ordinary life, and "ridiculous situations" would result if we did not do this. Since reality distortions "constitute the defensive system," they must be handled first. "In order to be dynamically and economically sound, the content of an interpretation need not involve symbolism, libidinal trends, sexuality, and so on; in delusional illnesses the appropriate dynamic interpretation may be merely an application of reasonableness and the establishment of a valid reality" (p. 148). The case material is developed quite convincingly along these lines.

Edwards & Peterson (20), in a brief paper on current psychiatric procedure in combat situations, argue that military psychiatrists have learned to "contribute a firmer discipline" (p. 721) with respect to military personnel attempting to avoid further combat via the medical route. They hold that the incidence of cases of "acting-out environmental manipulator" varies directly with the ease of attainment of medical evacuation (p. 723) and that 90 per cent of nonpsychotic Korean evacuees found in one United States general hospital could have been continued in effective service under the newer regimen. No quantitative data are presented; there is one illustrative case history.

Glass (37) points out that in World War I evacuation to rear hospitals usually yielded great resistance to improvement. By the war's end psychiatrists were employing brief therapy (a week or less) emphasizing rest, food, encouragement, suggestion, and persuasion in or near the combat zone, and achieving 60 to 75 per cent return-to-duty rates thereby (p. 725). Particularly, this extensive experience was not utilized during World War II but instead therapeutic efforts were concentrated upon discharge of anxiety through cathartic-abreactive techniques, commonly utilizing barbiturate interviews. "... only rarely could such cases be recovered for combat duty." (p. 727). Revival in 1943 of the World War I methods yielded a 50 per cent return-to-combat rate. During the Sicilian campaign cathartic methods applied within two days of breakdown salvaged only 15 per cent for combat duty. "Indeed, any therapy, including usual interview methods, that sought to uncover basic emotional conflicts or attempted to relate current behavior and symptoms with past personality patterns seemingly provided patients with logical reasons for their combat failure. The insights achieved by even such mild depth therapy readily convinced the patient, and often his therapist, that the limits of combat endurance had been reached as proved by vulnerable personality traits. Patients were obligingly co-operative in supplying details of their neurotic childhood, previous emotional difficulties, lack of aggressiveness, and other dependency traits . . . that displaced onus . . . to remote events over which they had no control and, therefore, could not be held responsible" (p. 727).

By 1944 it was becoming recognized that the mechanism of group identification operates in combat to counteract the fear for self and that removal from the group weakens the group forces and strengthens the self-preservative

ones (p. 729). "Simple methods of psychotherapy that stimulate and encourage positive feelings for the group are far more efficacious than any complex or time-consuming treatment which inevitably promotes self-needs and brings forth dependent character traits . . . repeated success of brief forward treatment demonstrated the need for repressive or suppressive therapy rather than uncovering depth techniques . . ." (p. 729). As a result of these developments the psychiatric methods in the Korean campaign were of this sort. Statistics on this campaign are not given, although it is stated that 40 per cent of psychiatric casualties assigned to noncombat positions in Japan were returnable to combat within three months, with "relatively few" instances of recurrence. It is pointed out that the method requires strong group identification by the psychiatrist, and that psychiatrists working far from combat have difficulty in applying the method because of their own guilt and their identification with the patient's need for escape rather than with the combat group.

Peffer (67) describes the rehabilitation of psychotic patients using money as a reward for healthy behavior. The idiosyncratic reward-systems of psychotic persons render it difficult to find a powerful incentive which will have some generality and be capable of easy transfer to the outside environment. The exchange power of money means that we do not have to fit the reward to the patient; he can select his own uses for it; money is rarely given for deviant behavior; most money-getting behavior demands some working with others. Peffer utilizes the VA¹ job "Member Employee" as a route for giving money rewards for improved behavior in chronic patients. The member-employee status is considered a stepping-stone to full community employability. He reports on 55 patients averaging a 10-year period of hospitalization; of these 6 had returned to the community and the other 49 were in this intermediate status. Clinical impressions of improvement in self-esteem and acceptance of responsibility are offered. This very promising approach should definitely be studied under circumstances of adequate control and follow-up.

Martin & Bird (56) describe the "stereoscopic technique" in which two therapists meet regularly to compare notes on two marriage partners, emphasizing highly charged episodes brought to the therapy. Considerable distortion occurs in such accounts and even with awareness of this danger, the therapist tends to be misled and to become involved in defending the perception of the episode gained from his member of the marriage. At times "these simultaneous views were so out of focus that justified doubts were raised concerning either the veracity of the patient or the powers of observation of the psychiatrist" (p. 124). Although the report is based on stereoscopic treatment of only four couples treated by psychoanalytically oriented therapy, it is a striking warning to therapists who think (or at least act as if they thought) that the therapist can make suitable corrections in the reality-portrayal of the patient, on the basis of his interview evidence alone. Although not collected in a therapeutic context, some data by White (an in-

ternist) on the effect of having a desk between doctor and patient are of interest (89). In taking medical histories of adult cardiac patients, the author interposed a desk between his chair and the patients' on alternate days. Using a fairly objective rule for classifying postures initially assumed by patients, he found that only 10.8 per cent of patients sat initially "at ease" when the desk was interposed, as contrasted with 55.4 per cent without a desk ($N=166$, $P<.001$).

Additional papers of some interest but presenting no data are those by Cutner (15) on "body experiments" in Jungian analysis; Farber (24) on written analytic sessions with a deaf therapist; Fliess (29) on recent developments in dream-analysis; Schneck (78, 79) on an interesting hypnotherapeutic technique of desensitization to charged situations through reconditioning while hallucinating the situation; Bonnell (9) and Mode (60) on problems in the therapeutic use of prayer; Havens (43) on the egocentric use of devotional reading; and Myerson (62) on the necessity of modifying the attitudes of relatives who reward the unhealthy behavior of alcoholics.

GROUP THERAPY

Only two experimental studies on group therapy could be located. Newburger & Schauer (64) studied its effects upon sociometric choices of reformatory inmates. Men were formed into matched experimental and control groups on the basis of their initial sociometric classification. For three months the experimentals met three times weekly in a therapeutic group, while the controls met in the library with no formal psychotherapy. After three months the regimes were reversed. Sociometric tests were made prior to the experiment, again at the end of the first three months, and again after the three months of reversed conditions. The sociometric data show an increase in the number of mutual choices following the therapy, which tended to be lost in the experimental group after the three months' discontinuance of therapy. Triangular structures tended to be replaced by chains. Both groups showed an increase in isolates following therapy, this rise being again lost in part when groups were discontinued. Group therapy "appeared to foster group cohesion" whereas exposure to the institutional regime without it "resulted in a decrease of mutual bonds and thus in lower group cohesion" (p. 13). Institutional atmosphere has a deteriorating effect upon chain development which group therapy can offset. It also appeared that institutionalization per se contributed to development of isolates, regardless of group therapy. Ratings on the Haggerty-Olson-Wickman Behavior Rating Schedule indicated that "in almost every instance the loss of extreme rejections and the establishment of reciprocated bonds of attraction coincided with marked behavioral improvement" (p. 15). "In no instance was it noted that marked improvement in the sociometric condition of the individual occurred without improvement on the behavioral level" (p. 15). These behavior ratings were made by cottage officers uncontaminated by data from therapy sessions or by knowledge of the sociometrics. The authors summarize several kinds of data, quantitative and qualitative, by asserting that

group psychotherapy thus appeared to have intensified, and at times started, a natural process of reorientation which became visible sociometrically in the form of an increase in the extremes of the social acceptance-rejection scale. Another . . . finding appeared to be the breaking up of more complex sociometric structures as an immediate effect of early group psychotherapy in institutional living (p. 17).

Talland & Clark (84) collected judgments from 43 patients in group analytic psychotherapy (seven groups) as to the therapeutic value of 15 "topics." There was good agreement as to the relative value of the topics, carrying over from group to group ($P < .01$). There was a high positive correlation between rated "helpfulness" of a topic and ratings of its "disturbing" qualities (.78 for group, .79 for self). Thus, the topic called "shame and guilt" was judged as highly disturbing, and group discussion of it as very helpful. When 35 psychologists rated the topics for "intimacy," the mean intimacy ranks correlated .69 with patient judgments of topic "helpfulness." The authors suggest that their data do not support the claim of some group therapists that which topic is discussed is largely irrelevant.

Corsini (14) uses a "behind-your-back" tactic in which the group discusses a member as though he were absent, thus simulating the important "gossip" feature of natural groups. Klapman (46) defends a more intellectual approach to some groups, using textbook reading to orient discussion among "poverty-stricken personalities" who otherwise react chaotically. He also describes a "psychiatric social club" (47) in which the original group therapist, a busy psychiatrist, fades out in favor of a group-elected nonprofessional member, the group continuing with a diversity of functions chiefly aimed at desensitization to the "psychiatric stigma."

Merry (58) advocates "excitatory group therapy," deliberately arousing tension, hostility, and anxiety in the meetings. Clinical impressions as to group therapy with various special types of cases are presented in articles by Armstrong *et al.* (2) on palsied adults, Cholden (12) on the blind, Kotkov on delinquent girls (51) and the obese (52), Laughlin (53) on analytic group therapy with executives, Parsons (66) on group therapy in private practice. A survey of the literature and discussion of theory is offered by Goldfarb (38).

THEORY AND "PHILOSOPHY OF THERAPY"

Whether it is possible any longer to distinguish between "psychoanalysis" and "psychoanalytically oriented psychotherapy" is discussed by Alexander (1). Since the theoretical concepts are identical, the recent modifications in psychoanalytic treatment have made any distinction difficult. He rejects such marks as interview frequency, duration of treatment, and use of the couch. Assessment of quantitative factors in the individual case must decide these questions, and the theory of this assessment is the same for "psychoanalysts" as for any competently trained psychiatrist. He argues that a more meaningful distinction is between primarily supportive and primarily uncovering methods. He notes that supportive measures are "knowingly or inadvertently used in all forms of psychotherapy; and, con-

versely, some degree of insight is rarely absent from any sound therapeutic approach" (p. 115). A therapeutic approach which is primarily supportive involves: (a) gratifying dependent needs; (b) abreactive opportunity; (c) objective review of the patient's situation from a "proper perspective," thus "assisting his judgment" (p. 116); (d) buttressing neurotic defenses; and (e) manipulation of the patient's life situation. Alexander deplors the tendency to assume that supportive measures are suitable for therapists with less technical and theoretical preparation than is needed for practicing psychoanalysis. It is puzzling why he still defends the necessity for some "trade-mark" to identify the psychoanalyst as such, "since the psychoanalyst can no longer be easily identified by the external criteria of his technique" (p. 118). He defends the orthodox analyst's insistence on, say, number of weekly sessions on the ground that "a product which cannot be easily identified by external criteria can be easily confused with other products which are essentially different but which appear similar" (p. 118). Up to that point in the paper, he has been making a very convincing case to the effect that the two products are not "essentially different," so long as both are (a) mainly uncovering rather than supportive, and (b) based on adequate dynamic formulations. It almost looks as though Alexander shares with his more orthodox colleagues the need to keep a monopoly on the prestigious term "psychoanalyst," in spite of the steady breakdown, both in theory and practice, of the distinction it once really made.

English (21) summarizes a symposium of analysts on the "essentials of (dynamic) psychotherapy" and how it differs from "classical analysis." Among the varied opinions voiced were: classical analysis mobilizes less anxiety in the therapist by providing standard patterns and requiring less evaluation and activity; "we keep assuming that psychotherapy is a watered down procedure or is bound to be pure psychoanalysis alloyed with the baser metals of suggestion, and so on (whereas) . . . really psychodynamic psychotherapy is an approach as strong as or stronger than classical psychoanalysis, has increasingly greater range of applicability than classical psychoanalysis; is more inclusive theoretically, and that classical psychoanalysis may turn out to be a special procedure of limited but significant usefulness in certain cases" (p. 550); lacking "objective measures" (p. 552), the setting up of therapeutic goals and strategies upon a basis of diagnostic assessment requires too much of the physician's subjective judgment; classical analysis is the treatment of choice if the patient is capable of it (Margolin, p. 557); goals of therapy "are expressive of the therapist's countertransference" (Shapiro, p. 558); nonanalyzed therapists are less flexible and "must therefore use a more stereotyped technique" (Margolin, p. 559). (One wonders what any adequate quantitative study of a fair sample of therapists would reveal!) He asks how can analysts do psychotherapy if the patient does not dream, free associate, or have fantasies? Gill (p. 560) pointed out that panel members had no trouble in deciding whether a given case was in "psychotherapy" or "classical analysis," and therefore some distinction should be

susceptible of formulation. He suggested a distinction in terms of aim rather than technique. "... one may say that psychoanalysis is directed toward uncovering the unconscious rather than toward therapy. Therapy is in a sense a by-product" (p. 560). This statement by a leading experimental analyst reads remarkably like the claim in Natenberg's polemic, *Freudian Psycho-antics* (63), that analysis is a method of psychological investigation conducted under the guise of therapy.

Zetzel (92) reports on a panel discussion of analysts on the problems of relating technique to therapy and of demarcating modified techniques from the "traditional." This fascinating give-and-take cannot be readily summarized, and the reader is urged to study the original. Most striking is the persisting disagreement upon fundamental issues within such a homogeneous camp. Is manipulation of the relationship the most effective way to bring certain needs into consciousness or is it inferior to "full analysis of the transference" along classical lines (p. 537)? Participants quickly realized the "far-reaching implications" of Alexander's justification of technical innovations on the ground that "facts are more important than words" (p. 531). How can one discriminate between "psychoanalysis" and "psychotherapy" when the former undergoes major modifications in technique? Waelder emphasizes a "vital difference" between his own "traditional" view of neurosis and that of Alexander; the former making the return of the repressed crucial in pathogenesis (a "wild life preserve" in the mind, p. 532) and hence viewing the lifting of repression as the essence of therapy. The technical measures advocated by Alexander appear to him to be rational upon an altered theory of neurosis, "attributing the illness not primarily to repressed impulses . . . but to a pattern of faulty interpersonal relations, resolution of which depends in the main on corrective emotional experience" (p. 533). The symposium reporter summarizes the issue as hanging on one point, "the extent to which analysis may be carried to a successful conclusion by verbal methods directed toward the acquisition of insight" (p. 537). A similar panel discussion of "defense mechanisms and psychoanalytic technique" (93) is far too complex for summary here, but is well worth reading for the light it sheds upon the current highly fluid state of psychoanalytic theory in relation to clinical practice.

Fisher (27), in a provocative dialogue-form discussion between two analysts, examines the perennial question whether therapy is an "art" or a "system." From discussing the value of prolonged supervision the protagonists move to the topic of how much knowledge, conscious awareness of strategy and of "manipulative technology" (p. 59), theoretical formulation of the case whereby the therapist is supposedly "several steps ahead of his patient" (p. 60), and the like, are either possible or desirable. The fundamental question, whether we do or do not now possess "a stable body of knowledge" (p. 61) concerning "human behavior," is reached halfway through the dialogue, and dropped within a few lines. This is, of course, the issue upon which most of the other disagreements between the two protagonists

hinge. In spite of some low-level dialogue about not being "mystical" but still having a "permeable skin," not suffering from "petrified convictions" or "too much knowledge," the paper is stimulating and makes some nice points on both sides.

Guntrip (41) thinks that "the therapeutic factor" is the relationship to a "good object," and that the therapist's own analysis is not mainly to learn technique but to become naturally a better object. Being a good technician is not being a good object, and being too afraid of personal feelings as countertransference is undesirable. "The patient has genuinely realistic needs toward the analyst . . . what he really needs is . . . non-erotic parental love . . . the analyst or psychotherapist must do for the patient what his parents failed to do" (p. 119). Lebo (54) in a brief paper traces the development of the "completely client-centered" orientation in the thinking of Rogers, using quotations from Rogers' publications over the period 1937 to 1952.

Thorne (85) presents further discussion of his eclectic "directive psychotherapy." " . . . a major therapeutic goal may involve the reorganization of a person's entire *style of life* through systematic modification of maladaptive core attitudes" (p. 274). Aware of the great danger and responsibility of his view, he unflinchingly employs the highly-charged phrase "thought control" to designate it. He distinguishes between establishing suitable conditions for learning (as in classical analysis) and "providing suitable training conditions according to the psychology of learning so that reconditioning actually takes place and is translated into action" (p. 277). He emphasizes the role of rational, intellectual activity in problem-solving behavior, and looks upon the therapist as "essentially a *master educator* who takes over where society, family, education and the person himself have failed to condition healthy behavior" (p. 278). Unfortunately for his stimulating position, he presents only the usual appeal to his clinical experience in its support.

Psychologists concerned with therapy training should read the paper by Grotjahn (40) on trends in psychoanalytic training. Problems of training and accreditation account for many of the schisms and rump institutes that look so odd in a discipline with scientific pretensions (49). The attempt to derive concrete technique from untested and sketchy theory results in a profession trying to rigidify the training even though its competent practitioners persist in rather fundamental disagreements. The danger of premature crystallization of training when we do not actually know what we are training for is one which a scientifically oriented profession such as psychology can presumably avoid.

There seems to be a growing interest in matters of "value"-orientation, reflected in such books as Rollo May's *Man's Search for Himself* (57) and Nuttin's *Psychoanalysis and Personality* (65), the latter being also a good general critique of Freudian theory and therapy. Weisskopf-Joelson (88) suggests that Western cultural bias undervalues a "strong, comprehensive, and consistent philosophy" as a contributor to normal stability and an aid

to therapy. She suggests that even therapies which theoretically repudiate any such aim as a therapeutic goal may in fact succeed partly because they really do subtly communicate the therapist's value-systems. She challenges the conventional view that persons who remain "clinically well" via their introjection of a philosophical, religious, economic, or political *Weltanschauung* are more prone to break under stress than others. As a final point she raises the question whether the very goals of conventional therapy (e.g., stress on "ambiguity-tolerance") may not be a "subterfuge of an unstable culture which has little to give but ambiguity" (p. 604).

Biestek (8) discusses the "nonjudgmental attitude" in casework and argues that the caseworker should "favor the good," because (a) of his social responsibility, (b) of his own integrity, and (c) the client will not be helped if the worker seems to subscribe to the "antisocial, illegal, or immoral attitudes or standards that brought trouble to the client" (p. 237). If the worker acts as though certain legal or moral prescriptions don't matter, a client who is in trouble or "feels guilty for his transgression" may conclude that the worker can't really understand or be of help. He emphasizes that there is a distinction between evaluating the client's acts and condemning the client as a person. He seems to be assuming that a therapist has to choose between "favoring good" and conveying that "it doesn't matter," which many therapists would not admit as the only alternatives. See also the stimulating paper on values and therapy by Ginsburg & Herma (36).

A new periodical, *Journal of Psychotherapy as a Religious Process*, has appeared under auspices of the Institute for Rankian Psychoanalysis. A rigorous, sophisticated consideration of the ethics of therapeutic "guidance" (such as Thorne's) by workers competent in axiology and casuistry is long overdue. Current thinking on this topic is almost wholly confined to clichés.

SUMMARY AND INTERPRETATION

The sparsity of research leads me to write a speculative, personalized summary. What will I as a therapist do differently as a result of surveying this literature? I find it hard to say. Perhaps my faith that therapy "does something" has been slightly strengthened (7, 19, 31, 32, 39, 50, 74, 81, 91). I cannot agree with those who consider this a foolish question or who feel little need to meet such challenges as Eysenck's (23). The history of the healing arts furnishes ample grounds for skepticism as to our nonsystematic "clinical" observations. Most of my older relatives had all their teeth extracted because it was "known" in the 1920's that the clearing up of occult focal infections improved arthritis and other disorders. No doubt the physicians who treated our ancestors by venesection had "observed" many "cures" in longitudinal study of their patients. Like all therapists, I personally experience an utter inability not to believe I effect results in individual cases; but as a psychologist I know it is foolish to take this conviction at face value. In order to bring about the needed research, it will probably be necessary for therapists and administrators to get really clear on this point: Our daily

therapeutic experiences, which (on good days!) make it hard for us to take Eysenck seriously, can be explained within a crude statistical model of the patient-therapist population that assigns very little specific "power" to therapeutic intervention. If the majority of neurotics are in "unstable equilibrium" and hence tend to improve under moderately favorable regimes, those who are in therapy while improving will be talking about their current actions and feelings in the sessions. Client and therapist will naturally attribute changes to the therapy. Furthermore, neurosis often shows cyclical fluctuations, and upswing terminators will be perceived as "successful," since therapists do not automatically find out when cases relapse or enter therapy with someone else [cf. Wilder (90, p. 329)].

How will such a statistical system be experienced by therapists? Very much as therapy admittedly appears to its candid practitioners. The best cases are types which seem most likely to improve anyhow. Sometimes there are temporal associations between improvement and interview events of the kind considered important, at other times such covariation is disconcertingly lacking. The therapist gradually conceptualizes the client, but there seems to be no clear-cut connection between the client's learning of this conceptualization and outcome. If the optimal treatment happens to correspond with the therapist's mode, causal connections are fairly clear. The therapist is like a Skinner-box rat on a schedule of intermittent reinforcement, which generates habits notoriously resistive to extinction. A sprinkling of even 5 or 10 per cent of "specific cures," cases whose shift toward recovery would not have occurred without intervention, could combine with the life-cures and the (unimproved) upswing terminators to yield the experiences therapists actually have in their daily work. Critics such as Eysenck deserve experimental-statistical answers.

Rosenzweig (74) criticizes Eysenck's survey on the ground that the treated and untreated groups were not comparable in severity, that the controls did receive some psychotherapy, and that the recovery criteria were not comparable. The paper definitely weakens Eysenck's attack, in spite of a somewhat obscurantist concluding section. But Eysenck's main challenge, Where is the good positive evidence?, remains untouched.

Outcome studies will make us more acutely aware of the differential treatability problem. In our present ignorance it is practically certain that clients are treated by methods of varying inappropriateness, largely as a function of which therapist they happen to get to. Also it is practically certain that many hours of skilled therapists are being spent with unmodifiable cases, or in the use of techniques which are effective but unnecessarily time-consuming. Considering the state of our knowledge, we still do not seem sufficiently daring and experimental about therapeutic tactics. Even when practical exigencies force a certain amount of trial-and-error, doctrinaire views about therapeutic theory are likely to be left unquestioned. For example, workers reporting large-scale success with suppressive methods (which, let me add, I do not employ in my own practice) feel obliged to apologize

for taking such an approach (37, 86). Learning theory considerations might be expected to make us very receptive to techniques such as the "method of tasks" (44, 75, 91). But how many of us will seriously begin experimenting with it, even when we discover that its enthusiastic proponents can present just as convincing statistics and as edifying case reports as anyone else?

If the reader thinks I exaggerate the confusion, let him study the panel discussions of the near-orthodox analysts on theory and technique (21, 92, 93), then Alexander (1), followed by Pumpian-Mindlin on brief therapy (70), Guntrip on the therapeutic agent (41), Thorne on directive psychotherapy (85), Glass on combat psychiatry (37), Train on "flight into health" (86), Wolpe on "objective psychotherapy" (91), rounding off his education with Maeder on psychotherapy and pastoral care (55). The lesson would seem to be that we know so little about the process of helping that the only proper attitude is one of maximum experimentalism. The state of theory and its relation to technique is obviously chaotic whatever our pretensions. To take one of the most familiar examples, we still do not know the relation between the "lifting of repression" and a therapeutic outcome. Is it casually efficacious, or itself a byproduct of the crucial process, or is it largely irrelevant? Perhaps it plays a different role in different cases? If so, how are these cases to be distinguished? What is the learning-theory account of these differences and its implications for procedure? The plain fact is that 60 years after publication of *Studien ueber Hysterie* we actually do not possess tested, systematic knowledge on such a fundamental question as this one.

Even such a powerful and illuminating work as Dollard & Miller's *Personality and Psychotherapy* (18) suffers from the traditionalism with which we are all infected. It is a brilliant rendition, in the learning-theory frame, of a fairly orthodox view of therapy. It might be even more illuminating if the authors applied these principles without being constantly guided by the tradition. It seems odd that the learning formulation apparently suggested so few innovations in therapeutic strategy and tactics as appear in that admirable book.

With regard to process research, one hopes that reliable indices of such states as anxiety, resistance, resentment, relief, etc., will be developed so that the momentary interview condition of clients can be evaluated from relatively nonjudgemental counts on the protocols. Outcome research will be of limited value in the improvement of therapy until this can be done [cf. Bordin *et al.* (10)]. Since a specific tactic (e.g., dream analysis, strong reassurance, pure reflection) cannot be fairly tested by a therapist who doesn't believe in it, rarely employs it, and hence has never developed any skills with it, the piecemeal experimental introduction of such tactics is not likely to prove very illuminating except in the hands of genuinely "uncommitted" skeptics and eclectics; but these, for all we know, may suffer from other defects, e.g., low zeal.

This leaves me with the now-familiar suggestion that a "statistical" study of therapy *in situ* is in order, taking different therapists more or less

as they come. There is an urgent need for more quantitative studies such as Murray's (61) of therapy other than by Rogerians, who to date are almost unique in their scientific efforts at self-study. But sooner or later we need a coordinated large-scale research program in which the range of theory and tactics should be widely sampled, designed so as to yield information about the higher-order interactions and the (no doubt tremendous) individual differences among therapists "officially" homogeneous in views. Multiple criteria of outcome should be used, at least until we know a great deal about the criterion intercorrelations and their post-therapy course. In this connection, the obvious merits of Q-sort should not lull us into thinking that the "hello-goodbye effect" (42, p. 228) is eliminated by the stratagem of treating numerous self-reports correlationally. Findings such as those of Kogan, Hunt & Bartelme (50) and of Snyder's group (31, 87) highlight the criterion problem.

Even so, I am impelled to one strong generalization. In the outcome research area, we are very far from utilizing the tools already available. This is no time to be fretting over the lack of an ideal criterion, or resisting outcome research because we don't know which is the "best" criterion. A well-designed study using (a) Q-sort, (b) therapist ratings, (c) uncontaminated clinician ratings, (d) ratings by spouse, (e) MMPI could hardly fail to be illuminating, if the other aspects (e.g., control group, repeated follow-up, and a good sampling of the range of therapies and clients) were properly dealt with. It will facilitate matters when clinical researchers finally accept the fact that the Rorschach is, at best, a relatively insensitive device for such purposes [cf. Peterson (68) and references therein].

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COUNSELING¹

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INTRODUCTION

This chapter reports on developments in the field of counseling for the year May, 1953 through April, 1954. The chapter includes papers on counseling and psychotherapy (a) carried on in a nonmedical setting and (b) written primarily by psychologists. Meehl's chapter reports related work in the field of psychotherapy; the two chapters should be read together for an overview not distorted by arbitrary criteria for a division of labor. The division mirrors the current ambiguity of the fields of counseling and psychotherapy and thereby records faithfully their history.

We shall review articles and books on counseling under four major headings: (a) professional problems, (b) descriptive reports, (c) theoretical contributions, and (d) research contributions. A preference for quantitative studies will be apparent. Theoretical expositions are included when they are substantial enough to suggest new directions for research or practice. The numerous reports of programs and of single experiences in counseling are either noted in passing or omitted altogether. Studies of tests and evaluative methods are included only when the report covers counseling methods also.

PROFESSIONAL PROBLEMS

For the past several years counseling as a professional field saw rapid development. The current year is more nearly a year of consolidation. There is little to report in the area of professional change and development, so far as the published literature is concerned. This does not mean that nothing important is taking place, but rather that few new landmarks are evident.

Perhaps the most important event of the year in the professional development of counseling was the launching of the *Journal of Counseling Psychology*, under the editorship of Gilbert Wrenn. The field of counseling has sorely needed a journal to relieve pressure on *The Journal of Consulting Psychology*, to represent uniquely the field of counseling, and to give more substantial treatment to counseling problems than is provided by the *Personnel and Guidance Journal*. The new journal should do much to stimulate research and the development of theory in counseling.

Reflecting the growth of professional counseling and the clarification of the functions of the counselor, Division 17 of the American Psychological Association changed its name from the Division of Counseling and Guidance to the Division of Counseling Psychology.

Lines of conflict between official psychiatry and other professional groups

¹ The following abbreviations have been used in this review: MMPI (Minnesota Multiphasic Personality Inventory); TAT (Thematic Apperception Test).

who work with people in distress became more clearly drawn. Official psychiatry has issued a statement (41) which substantially pre-empts the field, on paper, and attempts to leave to the counselor, along with a number of other professional workers, the burden of proof that they are not practicing medicine. The realities of competence, social need, and survival on the basis of social utility will work on the side of the counselor in ultimately defeating this bid for professional hegemony. In the meantime, the American Psychological Association has adopted a policy statement of considerable importance with which every counselor should be familiar. It is called *Psychology and Its Relation with Other Professions* (1).

During the current year the American Psychological Association also published a bulletin of technical recommendations for the preparation of tests and test manuals (2). For the most part a review of the bulletin is beyond the scope of this chapter. However, we do wish to call attention to the direct counseling implications of the use of tests, and particularly to the section on interpretation (p. 9 ff). The authors of the report point out that accuracy in interpreting results is not the only consideration related to test interpretation. They add,

An equally important concern is the examinee's reactions to interpretations of his test scores. . . . The teacher who interprets the results of academic achievement tests affects the student's self-concept and future learning. The clinician, in making interpretations which bear upon the client's areas of conflict, may unwittingly intensify those conflicts.

In other words, a therapeutic sensitivity to the effects of affect-laden information should guide the counselor as he interprets tests to a client.

DESCRIPTIVE REPORTS

Much of the literature in any one year is descriptive in nature. Its purpose is not so much to introduce new concepts or hypotheses as it is to describe some ongoing program or to re-interpret current concepts. Much of this literature serves the function of teaching rather than discovery. We shall touch upon samples of such literature in this section.

A booklet covering the second annual conference of administrators of counseling programs was edited by Berdie (9). In it there are papers by Gustad, Berdie, and Pepinsky. Gustad deals with the question of the definitions of counseling, and underlines the current lack of clarity in defining the field. He suggests that a multiple discriminant may be required to distinguish counseling from neighboring disciplines, and proposes a definition of counseling. Berdie's paper deals with the ways in which a counseling bureau can maintain contact with the students and staff of the institution it serves, and with prospective users of the service in the community. Pepinsky makes the point that group procedures have a definite place in a program of counseling and encourages practice and research in methods of group counseling.

One of the factors pertinent to the acceptance and growth of guidance services is the kind of support which administrators will give to these services.

Hamrin (39) has written a clear and useful book on initiating and administering guidance services, a book designed mainly to acquaint principals and superintendents with ways of planning and initiating programs in the schools. Included in the book are chapters on initial planning phases, in-service education, the public's role in understanding guidance, and counseling.

Two texts published during the current review year deal broadly with student personnel programs including not only counseling but many other services. Woolf & Woolf (78) have written a well-balanced text based on existing research evidence. Arbuckle (3) gives broad coverage to the personnel phases of the college organization and indicates the many ways in which the extra-curriculum provides counseling possibilities for students. Woolf & Woolf take a relatively eclectic position, while Arbuckle's book is written more particularly with a client-centered emphasis.

There were two books more specifically geared to guidance in the elementary and secondary schools. A book by Hatch & Dressel (40) is a nontechnical guide to counseling services in high school, useful for teachers, counselors, and administrators. Knapp (45) deals with varied guidance needs (vocational, personal, educational, health) and writes also at a level which does not presume much prior technical training.

Tyler (75) has had many years of experience as a counselor. She has also kept informed of what is going on. From actual experience and from reading of theory and research she writes a generally sound commentary on counseling. She covers such topics as the counseling interview, the use of records, the use of tests and of occupational information, and the selection and training of counselors. Except for the last chapter, which deals with the evaluation of counseling, research studies are appended to each chapter rather than being integrated into it, which makes for easy reading. No effort is made at the development of systematic theory. Her position is eclectic and her book abounds with common sense solutions to problems. We would say that the book has little value to the beginning student in psychology, who needs a more systematic conceptualization of counseling, but that it would serve well the guidance counselor, teacher, or clergyman who needs practical guidance in the art of counseling.

One of the encouraging events in postwar years has been the trend toward cultural interchange in education between the United States and a number of foreign countries. These exchanges have gone on not only through the Fulbright Act, but through a number of other devices. Lloyd (46) describes a year-long series of Institutes on Student Personnel Services conducted in Japan by university faculty members from the United States. It was a co-operative enterprise made possible by joint American and Japanese efforts. Lloyd's book gives an account of the planning of the institutes, the content covered, evaluation of the project, and recommendations for future work. The unique values of the book lie in two directions: first, in its practical demonstration of truly functional international co-operation and second, in the insights it yields about the cultural and educational transitions taking place in Japan.

The area of pastoral counseling is not central to our review here, but it is worth while to indicate samples of the annual literature in the field because the articles often involve psychological aspects of counseling. For example, Walters (76) deals in detail with the philosophical compatibility between client-centered therapy and the Christian doctrine. His essential argument is that the naturalistic context and its implications for the sovereignty of the scientific approach makes client-centered therapy incompatible with Christian teachings. Parsons (52) in the same journal advances the view that theology and therapy need not be in ideological conflict, but can arrive at an integration through concepts of growth and health. Another article, by Carpenter (15), deals with the meaning of acceptance in pastoral counseling. These examples could be multiplied, and they provide strong indications of the contributions of psychology to an allied field.

Another neighboring field in which the literature continues to deal with questions about counseling is that of social work. Some of the literature is conceptual and descriptive, but there are research articles to be found also. In an article on casework and counseling, Aptekar (4) brings together some comparative views of the "diagnostic" and the "functional" positions in casework and suggests some possibilities for synthesis of the two viewpoints. Fox (29) describes a study dealing with the effect of counseling on adjustment in prison. Ten sociologists and psychologists were designated as counselors and saw prisoners for problems other than custodial discipline. The experimental group was compared with a comparable control group on a scale of prison adjustment. Items included work stability, officers' reports, work reports, misconduct reports, and the like. The adjustment scores of experimental and control groups were compared, and the resulting differences favored the counseled group ($P < .05$). Fox points out that the counselor system realigned professional personnel within the institution in a way that enabled inmates to use the counseling as a personal service separated from institutional requirements and routine.

THEORETICAL CONTRIBUTIONS

Somewhere between the literature which describes and that which investigates lies the theoretical-conceptual literature, the literature which poses new ways of looking at counseling and which begins the process of generating research ideas. We shall be concerned with articles and books of this type here.

During the convention of the American Psychological Association one of the programs was a symposium on counseling as learning. Papers by Combs, Shaw & Shoben were subsequently published (16). In his paper Combs argues that traditional learning theory provides too atomistic an approach to holistic problems, and advocates the use of a personality theory in which such learning theory plays a minor role. A more satisfactory basis for explaining behavior in psychotherapy, he continues, is perceptual field theory.

The essence of this position is that all behavior is a function of the individual's field of perception at the point of behaving. Among the major factors which influence this perception are time, the physical state of the organism, values and goals, and the experience of threat.

Shaw describes a position which he calls interactive conceptualization. He takes exception to the concept of a standard set of psychodynamics (e.g., Oedipus complex) and suggests that any variety of learning experiences may precede counseling and give rise to varied sources of disturbance. Within this variety, a key general concept is that of nonadaptive interaction, which can be subdivided into three types: resourcefulness anxiety, repression anxiety, and inhibitory deficit. Shaw suggests corrective learning principles based on extant theories, e.g., broader generalization of learning sets, emotional conditioning, and the principle of contiguity. Principles of this sort may be combined to provide counseling with a solid theoretical foundation.

Shoben takes the position that a fruitful understanding of therapy may come from a fusion of two sources: the neoanalytic theories, which explain the client's behavior patterns in terms of social learning; and learning theory, which deals with the principles of learning and reorganization. He defines the task of the counselor and the process of counseling in learning terms, as follows: the client engages in self-exploration, which is reinforced in one way or another by the counselor. Further, when the client brings forth distorted perceptions of the clinical situation or the counselor, the latter assists in the extinction of these perceptions by pointing out, without anger or punishment, the nature of the distortions. Finally, the permissiveness and honesty of the counselor provide security for the client and modified cues which hitherto were anxiety producing for the client.

In another journal Shoben (66) has an article entitled "New Frontiers in Theory." This is a theoretical discussion of theorizing about counseling. It is sketchy and well within already explored boundaries. However, it is more substantial than most of the articles published in the *Personnel and Guidance Journal*, which should be making a greater contribution to the understanding of counseling.

The Pepinskys (54) have written a book that puts us a notch ahead in our efforts to understand the counseling process. The book makes two contributions, one to theory and one to practice. On the theory side, the authors systematically translate one brand of learning theory to the counseling situation and do so more thoroughly and systematically than others who have assayed the same thing. On the practical side, the authors describe and illustrate their thesis that the counselor should be both counselor and scientist, that he should adopt in his work with clients the same set that the scientist does in his approach to any other problem in learning. The set involves an awareness of theory, accurate observation, the establishment of explicit hypotheses (in each counseling situation the construction of a "hypothetical client"), the checking of hypotheses against independent criteria, the willing-

ness to abandon unfruitful hypotheses and adopt new ones, and the willingness to eschew absolutes for a process of successive approximations to pragmatic criteria of the effectiveness of counseling.

Five approaches to counseling are identified: self-theory (Rogers, Raimy, Snygg, Combs *et al.*); psychoanalytic (Alexander and French are cited as theorists and practitioners, and the contributions of psychoanalysis to the work of Rogers, Miller and Dollard, and others are noted); the neobehavioral (Mowrer, Miller and Dollard, Shoben, Shaw, Magaret, Rotter, and others); trait-and-factor centered (Williamson, Darley, Paterson, Super, and others); and communications (Robinson, primarily).

The Pepinskys identify themselves with the neobehaviorists and proceed to elaborate Hullian theory to provide what they modestly call not a theory but "an approach to theory construction." This tentativeness along with a preference for understatement and an appreciation for the contributions of others working from different viewpoints characterizes the book and enhances its value as a text for advanced students in psychology.

That many readers will find the book unsatisfactory appears to us to reflect on our state of knowledge rather than on the effectiveness of the authors, who have done a thorough job with the materials they had to work with. The authors are self-consciously scientific, not only in their preference for a learning theory that has had the most rigorous if limited development but also in their concern for the logic of science. The result is that the book does not come alive clinically. Only three brief cases, reported in summary form, are cited in illustration of the approach advocated by the authors. Throughout the book there is a strong preference for theoretical and research studies (the coverage of this literature is excellent) and only an occasional bit of clinical material.

Here seems to lie the dilemma of the theory and practice of counseling in 1954. If one works deductively from any fairly rigorous theory, there is so much attenuation by the time he gets to the individual interview that the enterprise can be justified only as a token of what we may hope some day to accomplish. On the other hand, if he works inductively from clinical materials, he winds up with a theoretical formulation (such as those of Freud and Rogers) which is practical and useful but lacking in the degree of theoretical rigor attained in the hypothetico-deductive systems at their best. It should be noted that the authors are aware of this dilemma. They note "Both *self-theory* and *neo-behaviorism* suffer from our present inability to generate, from either approach, rigorously deduced consequences that would lead to confirmation of one and rejection of the other" (footnote, p. 216).

Super (69) has written an article which should be studied rather than read. It is not just another article on vocational counseling, but a programmatic statement which lays down a whole direction of theory and research in the psychology of vocational development. Its focal point is the concept of "career patterns," and its method the longitudinal study of the forces which shape the vocational history of the individual. Super points out that a major

emphasis in vocational guidance has been the development of techniques and instruments for the diagnosis of traits and aptitudes, to the end that vocational prediction could ensue. There is no argument against this method, except that its cross-sectional emphasis leaves out those understandings which can come from the long-term study of the individual's development. It is this dimension of study which Super's work will add. This article will be understood in fuller context if one reads also the research outline which Super reports later on in the same journal.

Samler (62) presents 11 sensible but unexciting postulates about the nature of vocational counseling. Misunderstandings of counseling theory continue to breed straw men. It probably does not matter too much if the resulting article is well-written and entertaining, as is a bit of polemic called *Socrates was not a Rogerian* (65). This has to do with the author's perception of nondirective teaching. He doesn't like it.

Further misunderstanding is evident in Hadley's (38) suggestion that "the non-directive technique is quite comparable with the inferred technique in advertising." Epstein (25) points out that use of nondirective theory by advertisers "would mean that the advertiser [would] not sell at all."

A thought-provoking article by Weisskopf-Joelson (77) maintains that the pervasive element of client-centeredness in most contemporary theories of counseling and psychotherapy may be determined by a cultural bias.

Modern Western culture might be characterized by an absence of a strong, comprehensive, and consistent philosophy which gives the individual an interpretation of the purpose of life and a set of values by which to live. . . . The emphasis on the development of the patient's inner potentialities as compared to the imposing of values from the outside might be interpreted as making an asset out of a cultural liability.

The author suggests that psychotherapy should provide the confused individual with an appropriate and coherent life philosophy which might help him survive in "an unstable culture which has little to give but ambiguity."

Bach's (6) book, *Intensive Group Psychotherapy*, is an important contribution to the rapidly growing literature in this field. Bach is a psychologist in private practice and reports on his experiences in group treatment of neurotic adults. Bach uses open groups, meeting once or twice a week for 2 hr., with a post-meeting session for 2 hr. without the therapist. At one period in the five year history of a group, one person had been in the group for 16 hr. and another for 860 hr. The longest group participant also received 303 hr. of individual therapy. These figures suggest what is meant by intensive therapy but do not tell the whole story, which must be gained from a detailed reading of the text. Perhaps the most important central idea is the skillful use of all experiences, including the individual therapy sessions, to intensify and make more meaningful the group experience. Theoretically the book reflects the influence of Freud and Lewin, with the addition of a number of original ideas, particularly with regard to treatment techniques. The approach is eclectic, with little emphasis on systematic theory and almost none on quantitative

analysis or appraisal. But it is an informative book, literate and well-written.

The field of group counseling seems to invite even greater variability in methods than does individual counseling. Not only does it reflect the variety attributable to major extant theories of therapy, but it shows also the influences of sociometry, group dynamics, and educational practices. Group counseling is, in a sense, a gathering point for ideas rooted in psychology, sociology, and education. Perhaps, then, diversity in method, purpose, and even definition of group counseling is inevitable. A recent book by Driver (21) underlines the different ways that counseling in groups can be carried out and the different meanings of the term itself. Her procedures include seminar-type discussions, role-playing and sociodrama, self-evaluation, and unstructured discussions. One gets from the book a feeling for the many practical ways of organizing and leading a group, and this is clearly the major purpose of the book. What one misses is a systematic exposition of the rationale for the methods, or a statement of personality theory considerations which prompt their use.

The counselor who uses group techniques will find a very informative symposium on countertransference in group psychotherapy in the October, 1953 issue of the *International Journal of Group Psychotherapy*. Though prepared primarily by analysts working in a medical setting, a note on the issue is included here because of the relevance of the material to the work of the counselor. Flescher (27) identifies different manifestations of countertransference; Slavson (67) identifies in the group situation events likely to provoke positive, negative, and ambivalent transference, and what he terms "aim attachment" (the need of the therapist to set an aim and a direction for a patient or a group); Loesser & Bry (47) make observations on the total impact of the therapist on the group; Grotjahn (35) particularly points up the differences between transference in group and individual psychotherapy; Hadden (37) notes how the group can help a therapist become aware of his transference reactions and how observers with whom the therapist may later discuss therapy sessions may help; Schindler (64) points up the temptation to the therapist to assume a conventional father role in a group and manifest aggression or favoritism; Rosenthal (60) describes how countertransference manifests itself in activity group therapy; and Frank (30) presents a summary of the literature on countertransference. The psychologist reading the issue may temper his impatience at the lack of experimental data (even in an article labeled "an experimental study") by recognizing the rich supply of hypotheses inviting quantitative verification.

RESEARCH CONTRIBUTIONS

Research studies can be organized and considered from a number of standpoints. We shall group the research articles and report them under the following headings: (a) the counseling process, (b) outcomes of counseling, (c) client reactions to counseling services, and (d) the counselor.

While this method of organization has merit, it separates different phases

of a single research program. This was the case for the work by Snyder and his students. Those who wish to reconstruct the program as a whole may do so by considering the papers of Aronson (5), Blau (10), Gallagher (31, 32, 33), Gillespie (34), Kahn (43), Page (53), Peterson (55), Roshal (61), and Tucker (74).

The counseling process.—There are several ways to approach the study of the counseling process itself. One type of investigation may seek to determine changing patterns of client responses as counseling proceeds, another may compare clients at some given point in counseling to ascertain individual differences among clients, while still a third type may consider the question of variability in method among counselors. Dipboye's study (19) is an example of this third kind. The study raised two specific problems: Do counselors vary their styles or response patterns according to the topic under discussion? Do different counselors differ in technique within any given type of topic under discussion? The basic data of the study consisted of 51 verbatim interviews, representing the work of six counselors. Each interview was analyzed according to topics of discussion (interpersonal relations, self-reference, test discussion, etc.) and types of counselor response (questions about content, questions about feeling, interpretation of content, interpretation of feeling, etc.) The most general finding was that there was significant commonality in response patterns among the counselors. Nevertheless, differences also emerged. Dipboye found that four of the six counselors tended to vary their style according to the topic under discussion. It was interesting in this connection to note that the discussion topics formed two clusters, one of which drew primarily affective response categories from the four counselors and the other cognitive response categories. Thus, for example, interpersonal relationship and self-reference topics were likely to be accompanied by counselor attention to feelings while topics like test discussion and study habits were accompanied by counselor content responses. The other two counselors showed relatively little response variation among topics.

Since this study was not intended to draw validity implications about the effectiveness of these response patterns, we cannot say what these effects would be. A logical extension of this discovery about counselor differences of the kind described here would be a study which deals with the consequences of such differences to the outcome of counseling.

Investigators who come personally to terms with research in the therapeutic process find that a number of technical problems must be met and solved before such research can proceed effectively. Bordin *et al.* (12) present some of these problems in a methodological paper on therapy process research. The writers discuss such questions as the dimensionality of ratings, the methods of presenting the basic data for judgment, the effect of context on judgments, and the training and attitudes of raters. The article makes clear anew why therapy process research turns out to be a painstaking business and why researchers do not just "whip out" studies in the therapeutic process.

Among the designs for investigators is a proposal by Butler (14). He suggests an inductive approach to the study of counseling, using factor analysis techniques, and he presents a model for its use.

One of the technical problems in interview research concerns the optimal sizes of the units to be analyzed or classified. Muthard (51) raises the question of the sensitivity of large units in terms of their capacity to differentiate within and between interviews. He used three types of units: discussion topic, problem-area, and fraction (i.e., percentage of given remarks within an interview series), and rated 36 interviews using these units. He thus had three different ways to classify the identical content. He found that discussion topic and problem-area units showed greater sensitivity to variations in behavior than did the fractional unit. Implications were drawn for the use of psychologically meaningful units in interview research. Anyone who contemplates doing a study of interviews would do well to consider this article as an aid to arriving at decisions about his own work.

The kinds of responses clinicians are likely to make in counseling interviews were compared with laymen's responses in a study by Phillips & Agnew (56). The hypothesis was that clinicians would use the "reflection of feeling" response more often than would laymen. The counseling response test consisted of 20 paragraphs, each containing a client statement and five counselor responses. The test was adapted from the earlier ones by Blockson & Porter (11) and from Porter (57). High school and college students formed the untrained group, while the clinician groups consisted of 21 clinicians with a minimum of five years' experience in psychotherapy and 28 graduate students. The trained group were not necessarily client-centered in orientation; most of the experienced clinicians, in fact, had had at least two years of psychoanalysis.

The untrained group divided their choices rather evenly among the four nonreflection types of response (evaluation, interpretation, probing, and support) and chose reflection responses least of all. For both the clinical groups, on the other hand, the reflection responses were the most frequently chosen of the five alternatives. Indeed, the experienced clinicians chose reflection more often than all other alternatives combined. The authors conclude that clinical skills are more than just a simple extension in knowledge of interpersonal relationships which any intelligent layman might possess, but represent more definitely learned activities.

The method used here represents a useful experimental device which could be extended in its application. One of its values lies in the fact that it offers a standard stimulus through which direct comparisons between groups can be made. Thus one could explore further problems in comparative methodology. The problems inherent in the method, as in most standard-stimulus methods, lies in the limitations it imposes in response variation.

That the process of psychotherapy is complex everyone seems to agree. Because of this complexity, the various methods of process analysis that have been developed in the last 15 years illuminate some aspects of psychotherapy

and add to our partial understanding of it but fail to flood the whole process with light. Another shaft of light, playing on the interaction between hostility and defense, is brought to bear by Murray (50) in the analysis of a single case, as an example of a method of analysis. Fifteen recorded interviews with a 24 year old male with neurotic symptoms were played back. Simple sentences or meaningful phrases were categorized as evidencing hostility or defense, or as irrelevant to these two categories. The hostility category was divided into six subcategories providing a gradient from persons intimate with, to persons removed from the subject. The defense category was divided into two subcategories composed of intellectual defense statements and physical complaints. Good interjudge reliability for categorizing was obtained. It was found that as hostility increases, defenses decrease, with hour-to-hour fluctuations showing a reciprocal relationship ($r = -.73$). There was evidence of the displacement of hostility, with safer objects being the initial recipients. "In learning theory terms, fear is extinguished in the displaced situation and these extinction effects are generalized back to the fear in the primary conflict situation." There was also evidence of a reciprocal relationship between intellectual defenses and physical complaint defenses, with evidence that interpretation of a particular type of defensive statement tends to decrease reliance on that type of defense. The analysis ties in with psychoanalytic theory and learning theory. Because we have so few detailed therapy process analyses from the learning theory standpoint, this one warrants particular recognition as a pioneer effort.

Rogers and others have maintained that resistance in therapy is partially attributable to counselor error; if the counselor is able to keep threat at a minimum, resistance should remain at a minimum. Gillespie (34) investigated this general hypothesis through an analysis of 218 client-centered interviews of 43 clients. In these interviews, judges were able to identify 5,003 signs of resistance. The short answer made up most of the signs of resistance (41.5 per cent), the long pause was second (13 per cent), and rejection of the therapist's statements was next (with 11 per cent). Frank antagonism toward the therapist was found most infrequently (0.2 per cent). In general, no significant relationships were found between resistance and the nondirectiveness of the interviews nor between resistance and outcomes of therapy.

Efforts to tie in phenomena observed in counseling with constructs arising from established theoretical systems outside the field of counseling should certainly be encouraged. Several such endeavors are reported in the excellent series of studies under the direction of Snyder (68). Using Lepley's concept of response variability, Page attempted to ascertain the relationship between variability in verbal interview behavior and counseling outcome (53). He used four measures of verbal variability and two of verbal productivity. Page found no relationship between his measures of variability and progress in counseling. Total verbal productivity was associated with gain. Intercorrelations among the measures of variability suggest the generality of this factor. Roshal (61) working with generally similar hypotheses,

divided the subjects into a clearly gained and an uncertain gained group and assessed the change in the Type-Token ratio from initial to final interviews. Significant results were obtained, lending support to the hypothesis that people who profit from counseling will increase in variability. Pushing further the studies of variability, Blau (10) found no relationship between the number of client responses and the outcomes of counseling, nor between the discussion of problems in initial sessions and gain. His improved group did show more evidence of relief from symptoms and more tendency to discuss plans; however, there appears to be some contamination here for both of these variables are components of the criterion for improvement.

Outcomes of counseling.—There would probably be high agreement that increase in self-knowledge is one of the relevant purposes of counseling. Several studies indicate the importance of this objective by using it as a criterion measure of counseling outcome. Johnson (42) investigated the effect of vocational counseling on knowledge of one's own intelligence, interests, and personality. Just before counseling each client filled out a questionnaire on which he rated his standing on each of these attributes and the certainty of his views about the ratings. After counseling and again one month later the questionnaires were repeated.

The results showed significant increases in accuracy for intelligence and interest estimates, and an equivocal trend toward increased accuracy in personality assessment. Increased certainty or confidence about the predictions was also observed to a significant degree. Correlations between accuracy and certainty were positive but low.

Torrance (73) describes a college guidance program in which self-evaluation by the student forms one of the bases for subsequent counseling. Entering freshmen were asked to estimate and predict their performance on scholastic ability, achievement tests, and college grades. An attempt was made also to determine how strong or defensive were the attitudes about these ratings. The students were invited for advisory interviews after five weeks for the purpose of re-evaluating their predictions. Out of this procedure arises the possibility for the development of more realistic self-concepts. Research findings in the program indicated that there was generally low accuracy in the original self-estimates; most of the errors were in the direction of over-estimation. At the time of re-evaluation, an increase in accuracy was observed. Regarding the most serious miscalculators, Torrance reports that clinical personality studies revealed them to be "plagued socially, economically, emotionally, and sexually by an exaggerated sense of vulnerability." The report was a descriptive one, and might well have presented a more rigorous quantitative basis for the conclusions that were advanced.

Berdie (8) reports a study which tested hypotheses about self-knowledge in connection with a college freshman program of testing and counseling. The freshmen are typically encouraged to take psychological tests and have registration interviews. The 180 students who participated in this program were divided into an experimental group and a control group. The tests of

the latter group were placed in a file inaccessible to counselors; the tests of the experimental group were made available to the counselors and the students' advisers, and the students themselves were invited in for counseling. Since the criterion of gain from counseling was increased knowledge by the students of their measured characteristics, both groups were asked to fill out forms which asked for self-estimates of personality test scores, Strong Vocational Interest Blank rankings, general aptitude scores, and average first-year grades. In these self-ratings, the groups showed no differences in estimating personality ratings or ACE scores. In estimating interests, the men in the experimental group increased in their ability to rate their own interests. Further group differences favoring the experimental group were found in predicting grades and in percentage of drop-outs from the college. Berdie concludes that the study supports the use of changes in self-ratings as a means of evaluating counseling.

A problem in studies of this kind is the degree of independence between the experimental variable and the criterion. One could make a case for the notion that students become more accurate in rating their measured scores because that is just what they talk about in counseling. Judgments about scores alone thus would not constitute independent evidence of gain. On the other hand, predictions of grades suggest a reality orientation which comes from a more nearly independent assessment of events. These kinds of data, together with differential drop-out rate, would seem to constitute the more cogent criteria of gain from counseling in Berdie's study.

During recent years the Rorschach test has been used frequently to evaluate changes during therapy. One of the issues in its use has been the relative efficacy of specific signs versus larger qualitative units of analysis. In some of the earlier studies the large-unit method has seemed more advantageous. Peterson (55) carried out a study which compared three methods of Rorschach analysis with a group of client protocols before and after therapy, with the purpose of determining the relative yield of each method of analysis. The methods were graded in degree of specificity, though all used relatively small units of analysis as compared with the total possible range of unit size. The results indicated that no predictions regarding relative efficacy of the methods could be upheld. However, the one exception suggests the desirability of further study along these lines; the only scoring system which showed significant gain after therapy was the one which used the largest units of analysis.

A more crucial test of the relative efficacy of scoring systems would involve the use of protocol pairs where clear differences existed to begin with. Then the question could be put in this way: Which scoring system brings out these differences most fully in relation to an acceptable external criterion of change? In the study here reported, the original uncertainty as to the existence of change makes it difficult to bring out differences attributable to scoring systems.

Dymond (24) investigated the adjustment status of clients before and

after client-centered therapy by means of a standard set of self-descriptive statements sorted by each client. To obtain a criterion measure of adjustment, several sets of judges were asked to sort the statements into two piles, representing those which a well-adjusted person should say resembled him and the well-adjusted person should say were unlike him. The client's adjustment score equalled the number of items sorted in the same piles as the judges' criterion sorts. Analysis of the sorts revealed that clients were significantly higher in their adjustment scores after therapy than before. This finding for the experimental group contrasted with that for the control group, where no gain was observed over a comparable time span. Dymond handled the problem of validity of the measure in two ways. She showed first that the clients' estimates of their adjustment corresponded significantly with counselor ratings of success ($P < .005$). Second, she pointed out that the follow-up sorts maintained their gains during the no-therapy period following therapy.

Dymond's study should be read not so much as a self-contained validity study of psychotherapy but primarily as a methodological description of a criterion measure used in therapy research. It is evident from this and other reports that the final meaning of the results will be understood better as part of a more comprehensive report by the University of Chicago Counseling Center staff (59), which will probably be out by the time this chapter is published.

Gallagher (32) reported on MMPI¹ changes as a function of client-centered therapy. Forty-one clients were seen for a median of five to six interviews. The MMPI was administered to each client before and after therapy and compared with other criterion measures of therapeutic change. MMPI scores of 202 nontherapy college students served as a normal group for general comparative purposes. Analysis of pretherapy and posttherapy mean scores for the clients indicated significant changes in six of the eleven MMPI scales. Five of the changes were in the expected direction of lower scores, while the K-score, interestingly enough, was higher after therapy. This is an intriguing outcome and raises again the question of what K means; in its original meaning, K was not defined as a substantive personality variable but rather as an indicator of test-taking attitude. The interpretation has grown that a high K implied a defensive attitude. However, evidence continues to indicate provocative relationships between K and other behavior components in such a way as to expand the interpretation of Gallagher's results. For instance, Sweetland & Quay (70) suggest after reviewing the evidence that K "may also be a measure of healthy emotional adjustment." Tanner (71) found that student teachers rated "superior" had significantly higher K scores than did student teachers rated "inferior." The final definition of K has yet to be determined by adequate empirical methods, but it seems clear that the concept of defensiveness does not account for all of the meaning inherent in K.

Returning to Gallagher's study, we may say that, in sum, it lends tentative support to the hypothesis that therapy can result in positive personality change. The note of reservation arises from the fact that the absence of a control group in this study obscures the meaning of the changes. There is no attempt here to belittle the difficulties involved in devising good control procedures in therapy research. In an out-patient clinic or counseling center particularly, it is a difficult task to find comparable control individuals. As therapy research increases we might hope for a pooling of test-retest data for specified samples so that each researcher need not repeat similar control procedures. In other words, we might try building up norms for particular kinds of test-retest situations.

In a further analysis of the data, Gallagher (31) scored the MMPI data in terms of the Taylor Anxiety Scale, the Winne Neuroticism Scale, and the Welsh scales for anxiety and internalization. All of the scales showed significant score gains after therapy. There is some indication that these scales discriminate therapy change more efficiently than the original scales, though this is by no means definite in the data as they stand. One might think of this more as a useful possibility worth exploring further.

In the section on the counseling process we reported several studies in response variability. Rakusin (58) tested similar hypotheses with noninterview materials. He hypothesized that variability of response on the Rorschach (a) would differentiate students seeking therapy from students in general, (b) would be related to the amount of insight, planning, and variability of attack on problems in therapy, and (c) would be predictive of relief and of resolution of the client's problems. None of these hypotheses was confirmed. However, response variability within the counseling sessions was found to be predictive of relief and of resolution of the client's problems.

Kahn (43) concerned himself with generality as a personality trait manifested in response to the Rorschach and in counseling. Negative results were obtained throughout.

A rating scale for assessing the outcomes of psychotherapy is proposed by Morse (49). The scale covers six areas: symptoms, accessibility to therapy, occupational adjustment, school adjustment, sexual adjustment, and family adjustment. No reliability or validity data are given.

An important problem in counseling practice is the extent to which it is possible to predict outcomes for particular clients. Mitchell, Preston & Mudd (48) address themselves to this problem. They sought to determine whether the content of the initial interview in marriage counseling is predictive of developments in subsequent interviews. Records of 94 male clients were analyzed. Initial interviews were rated for six rather global elements ("counselor's impression of client," "client's handling of blame in marital situation," etc.) and subsequent interviews with a more extensive set of categories. The client's initial perception of his spouse was found to be a relatively effective predictor of subsequent developments. Clients with predominantly favorable

attitudes toward their spouses continue to express favorable feelings, are less likely to become involved in complex marital problems, rarely consider divorce, express less hostility in the interviews, and end with a more favorable attitude toward the counselor. The way in which blame is handled is also significant. "Favorable prognosis is associated with clients who accept a share of the blame but do not accept all of it." This is a nice study in a field where interest in quantitative verification is low.

Studies of failure in counseling are likely to be very instructive, but they are also very rare. Casual "experiments" reporting a few cases with client testimonials as evidence of gain abound in the literature, but we seem to avoid systematic analysis of our efforts when the clients do not remain long enough to testify favorably. What are the differences between clients who drop out before the third interview and those who stay for four or more interviews? Gallagher (33) studied approximately 80 clients, about half in a drop-out group and half in a stay group. His hypothesis was that "the more anxious the individual, the more likely he would be to remain in therapy, and the more defensive the individual the greater the likelihood of his escaping therapy." The Rorschach, the MMPI, and the Mooney Problem Check List provided his predictor variables. One highly significant result: both the drop-out group and the stay group were markedly more maladjusted than a control group of students not coming for therapy, thus indicating need for help on the part of those who are unable to stick with their counselors. The plausible hypothesis advanced by Gallagher gained some support, though tentative. There was some evidence that the more anxious group stayed in therapy and that the more defensive group left, but the lack of correlation among the different measures of defensiveness demands caution in accepting confirmation of the latter hypothesis. We need more such studies as Gallagher's. From what we know of counseling we can surmise that each report of a successful undertaking would involve in one way or another a quota of failures; perhaps it should become standard procedure to report as fully on these as on successes.

Cox (18) was interested in establishing sociometric measures as sensitive indicators of change in personality as a result of play therapy. Working with 52 children, ages 5 to 13, in an orphanage, he obtained a composite adjustment score based on sociometric ratings, TAT¹ scores, an adjustment questionnaire, and interview data from adults responsible for the care of the orphans. The sociometric choices correlated with the composite adjustment score about as well as did the TAT scores (.76 and .80 respectively). But it should be noted that the composite score included these as part scores. The author concludes that the "TAT and sociometric ratings were reasonably accurate indicators of these children's adjustment."

Two groups of nine children each were selected and matched for important variables including the composite adjustment score and TAT and sociometric ratings. The experimental group was given nine weeks of play therapy.

Both groups were measured immediately after the therapy period and again 14 weeks later. Both the TAT and the sociometric ratings indicated improvement in the adjustment of children receiving play therapy. Cox concludes: "If confirmed, these results suggest that sociometric status may become a powerful tool in the assessment of personality."

Most studies of counseling end up with some uncertainty associated with the inadequacy of available criteria for assessing the effectiveness of counseling. Thus any direct attack on the criterion problem is welcome. A modest effort has been made by Tucker (74), who derived a weighted composite criterion based on a brief client posttherapy scale, a counselor posttherapy check list to be filled out by the counselor and independently by readers of his protocols, and a "positive-negative ratio" not unlike the "discomfort relief quotient" of Dollard and Mowrer. Low correlations were obtained between the client's scale and the other measures, leading the author to speculate that "the clinician's point of view on a client's progress refers to some 'psychological' value system quite independent of the client's everyday operational values."

Keet's impressive study of a few years back seemed to offer a neat technique for assessing the effectiveness of different approaches to counseling, particularly since Keet reported striking differences in results of an interpretive and a nondirective approach in favor of the former. Keet employed a learning test consisting of words from the Jung list, with clearing up of repression cued by a word as his criterion for therapy. In 1952 Merritt repeated the experiment, with negative results. Grummon & Butler (36) tried it also and failed "to confirm Keet's finding of selective forgetting of traumatic words over the neutral words on the reproduction test." The breakdown of what appeared to be a very useful method for achieving a behavioral criterion of change in therapy is disappointing but points up the need for replication of critical studies in this field.

Client reactions to counseling services.—Forgy & Black (28) report a provocative follow-up study of client's reactions to two different methods of counseling. In the original 1949 study by Barahal, Brammer & Shostrom (7), the authors investigated differential client reactions to client-centered and "traditional" educational-vocational counseling methods. The 1949 study reported no significant differences on a multiple-choice questionnaire but found significant differences in feeling-tone favoring the client-centered group, as rated from postcounseling interviews with each client. The follow-up study inquired again into client reactions to this earlier experience, using a mail questionnaire designed to assay client satisfaction with counseling. The results were equivocal with regard to differential effects of the two counseling methods, some items favoring one method and other items favoring the second method. One would have to conclude that if there was a difference in 1949, the difference had dissipated by 1952. The 1952 study called into question the original procedure of identifying feeling-tone from the

postcounseling interviews and made a cogent case for the fact that these interviews contained information identifying the counseling method which had been used with each client. This would make possible the introduction of bias into the original ratings. One should not imply that bias was actually at work; however, so long as the possibility of bias exists, the results must remain inconclusive.

The follow-up study contained one further analysis which should be underlined here. The authors tested the possibility that there was an interaction effect between counselor and method and found that particular combinations of counselors and procedures tended to influence the satisfaction of clients, i.e., one counselor could produce more client satisfaction using one method and another counselor could produce more satisfaction using another. This finding continues to confirm earlier studies which have shown that effects more complex than method alone are at work in the process of counseling.

Dobson (20) reports a questionnaire study of client reactions to vocational service. The results were based on a 60 per cent return. In addition, a number of former clients could not be reached. Any results of course must be considered tenuous on this basis. Of the clients answering 83 per cent reported that they felt helped by the agency, primarily in adding to their understanding of their own abilities and in gaining self-confidence. About a quarter of the clients stated that they had made specific changes in vocational plans as a result of counseling. We need not labor obvious points about the limitation of the study. Perhaps the chief point worth noting is the incidence of reported gain in sense of adequacy; if such results appear in more rigorous studies we shall have further leads on a most important by-product of increased self-knowledge.

Still another inquiry using the questionnaire method to evaluate guidance is the study by Braden (13). Here also there was an exceedingly high proportion of nonreturns; fewer than 40 per cent of the group returned their questionnaires. Little more need be said of the study, except that increased confidence in their vocational adequacy received prominent mention.

The counselor.—Among the many facets of research in counseling, one of the least-studied phenomena is the counselor's understanding of his client. Dymond (23) studied the problem by raising this question: To what extent can counselors predict their clients' descriptions of themselves after therapy? There were two main steps in the initial phases of her procedure. First, each client after therapy sorted 100 self-descriptive statements; second, each counselor was asked to sort the cards as he predicted his client would. By the simple process of correlating each pair of sorts, it would be possible to determine the degree of correspondence between the client's sort and the counselor's prediction.

The problem, however, is not quite this simple. Experienced counselors learn about clients in general, and their predictions could come out of this

general knowledge rather than specific understanding of the individual client. Dymond took this into account by asking each counselor to sort for the "typical post-therapy client." Her question in a revised form then was, "Do counselors predict accurately for their individual clients when their general knowledge (stereotype) is partialled out?" Her results were as follows: (a) the median prediction accuracy is represented by a correlation of .41; (b) when the stereotype is partialled out, half of the predictions remain significant at the 1 per cent level. She concluded that there was significant evidence of "individual prediction" or understanding of the client.

Additional evidence that personal adjustment is related to the ability to predict responses of others is presented by Tarwater (72) in a minor but useful study.

Cottle & Lewis (17) report a study of differences between counselors and a college student sample on responses to the MMPI and the Guilford-Zimmerman Temperament Survey. The ultimate purpose of the work is to construct a scale which differentiates between counselors and other workers in education and psychology. The present study yielded a pool of over 100 individual items which discriminated between counselors and students; it also pointed out which of the existing test scales discriminated between the groups. On the MMPI, the counselors were significantly higher on K, significantly lower on the Lie and Manic scales, and more socially extroverted (Si). On the Guilford-Zimmerman, the counselors scored higher in adjustment on 6 of the 10 scales.

In the long run the group differences in these scale scores will be less consequential for the authors' purpose than the pool of differentiating items which they found. This is particularly true because there is no way of knowing how representative of students-in-general this particular sample is. Whether these items turn out to be useful in distinguishing between counselors and other professional workers is a matter for cross-validation. The authors indicate that this step is already in progress.

A study by Aronson (5) investigating the relationship between counselor behavior and client behavior revealed that counselors differ among themselves but that these differences were not significantly related to client behavior. Accompanying these negative results was one positive and most intriguing finding: the counselors who relied most heavily on nondirective techniques were most able to develop warm social and personal relationships and were most easily upset, anxious, submissive, and dependent, in the judgment of their colleagues. And there was some evidence that the counselors "who ranked highest in intellectual curiosity, achievement, and exploratory interests relied least on nondirective techniques in therapy." The tender minded and the tough minded again. The nondirectivists' interest in research, of which Aronson's paper is an example, must simply be reaction formation!

EVALUATION OF THE YEAR'S LITERATURE

In considering the year's published literature in counseling, we see no one striking trend but rather a series of modest but solid advances. The research studies, though uneven and naive in spots, nevertheless are guided increasingly by concepts drawn from psychology in general. This is a healthy trend. It means that counseling studies draw their necessary sustenance from the basic psychology from which counseling was derived and to which it must remain linked. The studies which we have especially in mind in this connection are those by Murray (50), Rakusin (58), Page (53), and Roshal (61) and the methodological article by Bordin *et al.* (12). The reader may recall that the studies dealt with such concepts as variability in verbal behavior and the interaction of defense and hostility. The fact that some of these studies deal with analysis of the counseling process signifies that we are getting some advance also in the breadth of interview research. This is especially welcome in an area in which counseling research had an early start, but which has seen few new developments recently.

An analysis of the studies in outcomes of counseling can be considered from two standpoints, one relating to substantive findings, and the other relating to methodology. In terms of the substantive findings, evidence continues to accumulate that counseling produces changes in predicted directions. We can quarrel with any one study and point out real limitations and even defects. The cumulative evidence is nevertheless too consistent to be dismissed. If we had to say yes or no to the question "Does counseling produce change in the desired direction?" we should have to say yes, recognizing at the same time that there are many more questions we still need to ask about counseling outcomes.

With regard to the question of methodology, we have already commented on the general unevenness of control procedures. This is of course not true for all studies, but represents a point which still needs attention. Concerning the very important question of criterion measures used in the outcome studies, we find that a variety of such measures have been used but that few new extensions have been made this year. There is still much room for development here. A number of studies, such as those which use the Rorschach as a criterion, leave some doubt as to what is being validated, the counseling process or the instruments used in assessing it. On the other hand, it may be expecting too much to ask that the Rorschach reflect change in very brief therapy, and so we have the question of the pertinence of the criterion for its particular purpose.

The scope of studies during the year is interesting to consider. There was only one completed report of programmatic research, that carried out under the direction of Snyder. This constitutes a notable lack in a field where research planning of such scope is so badly needed. The sheer task of getting enough usable data in counseling studies and the necessary length of the ex-

periments emphasizes the importance of group research in this field. Super's maturely conceived, long-range study of career patterns, already underway but to be realized over a period of years, may be cited as a model of programmatic research and of research leadership in the field.

There have been no notable advances in counseling theory during the year. The Pepinskys' book represents a useful synthesis of efforts, which have been increasing in recent years, to effect a rapprochement between counseling theory and learning theory. The symposium by Combs, Shaw, and Shoben also should be noted in this regard. Though these efforts at present seem to be long on theory and short on clinical vitality, they represent one of the more promising trends in the counseling field.

There have been only a few books which claim to present new thoughts on counseling. When a book appears which does introduce new ideas on counseling, or which recommends new procedures, we usually raise the question, "How good are these methods?" The whole essence of a new presentation is that it is introductory and cannot be expected to marshal much evidence to answer this question in the beginning. We probably should not demand much concrete evidence the first time around on such ideas. Nevertheless, it would seem to be a healthy thing for writers of such ideas to indicate at what point on the time-line of verification they think they stand. It would help, in other words, to see evidence that writers have one ear cocked to the necessity of evaluating new ideas at some time. On this score the new books show wide variability, ranging from implicit contentment with clinical validation to recognition for the usual research methods of verification.

On the question of counseling as a profession, the literature suggests that the year has been a quiet period. When our professional efforts are minimal, perhaps we can turn more of our attention to counseling as a science. In this regard, the counseling literature for the year may have anticipated the American Psychological Association Board of Directors' hope that "We . . . are in a position to examine reality for ways in which the Association can appropriately turn more of its resources to the advancement of psychological science" (63).

There are several more scattered observations that we might make here. Interest in group psychotherapy continues high, though there is a conspicuous preference of workers in this field for the general expository article rather than for quantitative research.

A lot of thin material still gets published in the field of counseling. It seems that anybody who wants to sit down and write something about counseling can get it published. The reviewers rejected some 70 articles on the grounds of triviality.

Without evidence to cite in support of the observation, the authors believe that there has been a shift in attitudes toward psychologists doing counseling and psychotherapy. Interprofessional controversies seem no longer to

focus on the issues, but rather on the circumstances of practice by the psychologist. And in this latter area, psychologists seem to be steadily strengthening their claims to equal status in a truly cooperative endeavor.

The appearance of the first issues of the *Journal of Counseling Psychology* should be recorded as an important event of the year which should encourage the publication of more substantial papers on counseling theory and research.

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STATISTICAL THEORY AND RESEARCH DESIGN¹

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INTRODUCTION

One of the more noticeable trends during the year under review is the increase in amount of literature pertinent to quantitative methodology in psychology. As a result, this chapter falls far short of representing a comprehensive survey of relevant papers. In the selection of articles for review, differential attention is provided those topics on which significant contributions have appeared. Foremost among these are statistical methods and factor analysis.

The increase in amount of literature is particularly dramatic in the statistical journals. The content of the 1953 volume exceeds that of the 1952 volume for each of the four prominent journals, *Journal of the American Statistical Association*, *Annals of Mathematical Statistics*, *Biometrics*, and *Biometrika*. Total increase in size of the four is 484 pages, a 22 per cent jump over 1952. Further, it appears that a high proportion of the articles have direct relevance to research methods in the social and biological sciences. Statistics has reached a stage of development at which many statisticians are alert for areas in which to suggest methodological advances. That psychology and other social and biological sciences provide challenging methodological problems seems certain. A belief that more efficient research design in psychology can result from knowledge of current statistical developments has contributed to the emphasis, in this review, upon statistical theory and techniques.

Factor analysis, one of the few quantitative methods predominantly developed and utilized in psychology, has enjoyed unusually valuable development during the year. There have been proposed practicable statistical tests for "when to stop factoring," and for testing the stability of factor loadings. Analytic methods proposed for other stages of factor analysis threaten to make less subjective the applications of this approach. As a consequence, the topic is treated with special attention.

During the period of review there have been published a large number of important books pertinent to statistical theory and research design in psychology. Among new statistical texts, at an intermediate or elementary level, are Walker & Lev, *Statistical Inference* (100), an excellent, teachable book; Linquist's valuable *Design and Analysis of Experiments in Psychology and Education* (60); an unusual introductory laboratory-oriented text by Townsend (96); and introductory texts by Lacey (55), Clark (16), David (23), Goedicke (35), and Connolly & Sluckin (21). Quenouille's text (75) presents

¹ The survey of the literature pertaining to this review was completed in May, 1954.

extensive illustrative applications of complex analysis of variance designs. Important contributions to the area of survey sampling are the text of Cochran (17), the two volume work of Hansen, Hurwitz & Madow (43, 44), and a new edition of a book by Yates (104). General contributions to quantitative methodology in psychology include Ackoff, *The Design of Social Research* (2), *Research Methods in Behavioral Sciences*, edited by Festinger & Katz (31), and the work of Stephenson on *Q*-methodology (87), more completely discussed elsewhere in the present volume. A book of tables useful to psychological researchers is that of Romig (77), tables of the binomial probability distribution for $50 \leq N \leq 100$.

ESTIMATION OF PARAMETERS AND TESTS OF HYPOTHESES

Current statistical literature attests to increasing awareness that the usefulness of conventional hypothesis testing methods is severely limited. The typical procedure of testing a null hypothesis against all possible alternatives, a procedure so prevalent in psychology, is often inadequate in several respects. One limitation is that stressed by Bechhofer (8). In experimental analysis, it is not unusual to test the homogeneity of a set of statistics. The hypothesis may stipulate that a number of different experimental effects yield the same (population) mean performance. An hypothesis of this form often is unrealistic, and results from the test may not provide the most useful information; for, if the effects really are different, then population mean performances also will differ, and a sufficiently large sample will establish this at any prearranged level of significance. Such considerations underscore difficulties in interpreting the "acceptance of the null hypothesis." Without considering a specific alternative hypothesis against which the null hypothesis is tested, and without considering both Type I errors and Type II errors for the employed test, little use can be made of information that the null hypothesis has not been rejected.

A second difficulty involves the too frequent failure to distinguish sample results which differ with statistical significance from those for which the differences are of practical importance. This problem might be relieved partially by the change in terminology proposed by English (30), who suggests that we replace "significance" by "stability" when speaking of statistically reliable findings.

A final problem for which several solutions recently have been proposed is that which arises after having rejected the hypothesis of homogeneity (e.g., in analysis of variance) when the investigator then wishes to specify the particular differences among sample statistics which safely may reflect parallel differences among parameters.

Some of the difficulties encountered in hypothesis testing procedures are alleviated if the investigator is able to test an hypothesis against a particular single valued alternative hypothesis. His decision then is based upon the relative likelihood of the two hypotheses, given specific empirical data. He is able to arrive at a decision, having chosen a most powerful statistical test.

However, it appears seldom that a psychological investigator is willing to specify in advance such an alternative hypothesis. Certainly, such a procedure should be encouraged; among other advantages, it serves to incorporate a criterion of practical significance in the statistical test. The value chosen under the alternative hypothesis typically can be the minimum value yielding a difference of unquestionable practical importance.

One other approach is suggested for the investigator who continues to feel unable to express a specific alternative hypothesis. He then would be misled less frequently and would be more likely to obtain the information he seeks were he to formulate his experimental problems in terms of the estimation of population parameters, with the establishment of confidence intervals about the estimated values, rather than in terms of the test of a null hypothesis against all possible alternatives.

Estimation and tests of proportions.—Gengerelli & Michael (34) indicate, correctly, that a confidence interval about a proportion, p , from a sample of size N , is precisely found by the quadratic solution for π in the formula

$$\frac{p - \pi}{\sqrt{\frac{\pi(1 - \pi)}{N}}} = k$$

where k is the normal deviate (critical ratio) associated with a particular confidence coefficient, $1 - \alpha$. Such precision is lacking in the conventional method, where the one estimate for the standard deviation of the population proportion must be derived from the sample p . The suggested quadratic method provides two estimates for the standard deviation, each associated with a value of π at one of the limits of the confidence interval.

For problems of testing the reliability of the difference between two proportions, Gengerelli & Michael illustrate and urge the use of the quadratic method as a substitute for the conventional test. Essentially, the proposed solution provides limits of the interval in which resides a population proportion consistent with the observed difference between two sample proportions, at a specified level of confidence. The two sample proportions, the sample sizes, and the correlation (if any) between the two samples are assumed known. If there is no population which could generate the obtained difference between proportions, the solution is imaginary. For independent sample proportions, the quadratic solution for π stems from the equation

$$\frac{p_1 - p_2}{\sqrt{\frac{\pi(1 - \pi)}{N_1} + \frac{\pi(1 - \pi)}{N_2}}} = k$$

where k is the normal deviate associated with the specified confidence coefficient.

The hypothesis tested by the Gengerelli-Michael procedure is that "there (is) a parametric proportion which could, at a given confidence level, yield the obtained difference between two empirical proportions." The authors

claim that "the usefulness of the method . . . is especially clear when the obtained proportions are near one end of the scale." On the contrary, it might be argued that the value of the method becomes more and more restricted as the observed $p-q$ splits become extreme. Consider the illustrative problem: $p_1 = .82$, $p_2 = .95$, $N_1 = N_2 = 100$, $k = 1.96$ (associated with a significance coefficient of .05), for independent sample proportions. A quadratic solution is found to be: $.33 \leq \pi \leq .67$. In this instance, since "the two values are real . . . the null hypothesis would be sustained" (34). Such a decision is entirely correct, since it has been demonstrated that, with $\alpha = .05$, there is a specified range of parametric proportions, any of which would yield an obtained difference of .13. However, the decision does not enable us to accept or reject, at any level of significance, the hypothesis that the two sample proportions have been selected randomly from the same population. (In the present example, it would seem unlikely that a population with a proportion no greater than .67 would yield two samples of 100 with proportions .82 and .95.) The example serves to illustrate the basic practical weakness of the proposed quadratic method, that it does not serve to test an hypothesis of homogeneity of two sample proportions.

It is instructive to note progressive developments over the past decade in the area of inspection sampling or quality control. In its most simple form, the problem faced in inspection sampling is the test of an hypothesis concerning a single population proportion. A sample is randomly selected from a lot, and a test is made on the sample to determine whether the proportion of defective items in the lot is sufficient to reject the lot. In designing his sampling plan, the inspector determines a sample size, N , and a critical decision value (acceptance number), c , which will attain simultaneously (a) the acceptance, with high probability $1 - \alpha$, of lots having a true proportion defective equal to or less than a specified acceptable value and (b) the acceptance, with low probability β , of lots having a true proportion defective equal to or greater than a specified objectionable value. Psychologists profitably could adapt this and similar models to their experimental designs, not only for tests on proportions, but for other statistics as well. For the particular problem outlined above, Grubbs (39) has presented tables for the determination of sample size and acceptance number. More recently, Golub (36) has presented a solution for the same problem, given a fixed small N . Golub solves for the value, c , which will minimize the sum of the probabilities α and β ; these "best" acceptance numbers are tabled for $5 \leq N \leq 40$, in steps of 5.

Claringbold, Biggers & Emmons (15) discuss the angular (arcsine) transformation of proportions, a transformation recommended to correct for association of proportions and variances of proportions in analysis of variance. Tables of the transformation, for $0 \leq p \leq 1.00$ in steps of .001, are presented by Stevens (88).

Estimation and tests of means.—An excellent article by Proschan (74) serves to clarify the distinction between confidence intervals and tolerance intervals, and exemplifies a number of differing cases of each for intervals

about means of the normal distribution. All such intervals have the form $m \pm k(\text{s.d.})$, where m is either μ or \bar{x} and s.d. is either σ or s , depending on whether or not the population mean or standard deviation or both is known. Two cases of confidence intervals presented are: (a) an interval, $\bar{x} \pm k(\text{s.d.})$, such that the probability is $1 - \alpha$ that the population mean is included; (b) an interval $\bar{x} \pm k(\text{s.d.})$, such that the probability is $1 - \alpha$ that a single, second sample mean is included. Two cases of tolerance intervals also are presented: (a) an interval, $m \pm k(\text{s.d.})$, such that, on the average (i.e., for the average of a large series of random samples), the interval will contain a specified proportion p of the population; (b) an interval $m \pm k(\text{s.d.})$, which, over a large series of repeated samples, contains a proportion p or more of the population, $1 - \alpha$ of the time. Each case is succinctly exemplified for various combinations of known and unknown population mean and standard deviation, and extensive tables for k are presented when $\alpha = .50$ (and $p = .50$) for numerous small sample sizes.

Consider the problem of finding a combined estimate for a population mean from several samples, where each sample mean may be considered an unbiased estimate of the same μ even though the samples may come from populations with different variances. For such a problem, it is typical to assign to the sample means weights which are reciprocals of the estimated variances. In a sampling investigation, Cochran & Carroll (18) have investigated the precision of this estimate, in comparison with the precision of an unweighted estimate for the case of two samples. They provide evidence that, if the ratio of the larger to the smaller population variance does not exceed 2, then the unweighted mean is the more precise estimate. If that ratio exceeds 3, the weighted mean is the better estimate. For ratios between 2 and 3, the weighted mean is superior if the computation of each weight is based upon at least 12 degrees of freedom. The variance of the weighted mean estimate, allowing for the sampling variability of the weights, is studied by Maier (64); from the sampling experiment of Cochran & Carroll, it appears that Maier's formula is satisfactory for sample sizes of 12 or greater.

When the purpose of an investigation is the specification of a confidence interval, of given size, about the sample mean \bar{x} , such that the probability is $1 - \alpha$ that the interval includes the population mean, a useful two stage sampling scheme is available. This procedure, attributable to Stein (86), provides economy of sample size, for the size of the second sample stage is dependent upon information about the population derived from the first sample stage. On the basis of Stein's paper, Seelbinder (85) provides tables useful for determining optimum sizes for the first stage samples at confidence coefficients, $1 - \alpha$, of .90, .95, .98, and .99.

Owen (69) describes a two-stage sampling plan for a test on the mean (easily extended to the difference between two means) of the normal distribution. The proposed test generally requires fewer observations to maintain the same power as the conventional single sample tests. The chief advantage over Stein's scheme is the provision for a definite ceiling on the number of

observations required. Use of the test does not depend upon the specification of a particular alternative hypothesis. Owen's test procedure is first outlined for the case of known standard deviation, then is extended for unknown σ . Facilitating tables accompany discussion of the proposed test.

In recent years, nonparametric statistics have become popular substitutes for Student's t in one sample and two sample tests of location. Lehman (58) investigates the power of rank tests, and Dixon (27) studies the power of sign tests for various classes of alternatives to the hypothesis under test. Dixon demonstrates that for samples from normal populations, the power of sign tests relative to the power of t decreases with increases in N , $1-\alpha$, and the distance between locations specified in the hypothesis under test and that specified in the alternative. David (24) studies the power of some tests based upon sample range which utilize tables of the Studentized range as a substitute for tables of F in single and double classification analysis of variance designs. In certain situations the range test is found to be more powerful than the conventional F test. Rosenbaum (79) presents tables of critical values for a simple two-sample nonparametric test of location. Assuming two samples, of sizes N_1 and N_2 , are randomly and independently selected from the same population, whatever its form, Rosenbaum provides tables for the probability that S observations from the sample of N_1 are greater (less) than the greatest (least) value of the sample of N_2 , for $N_1=1, 2, \dots, 50$, and $N_2=1, 2, \dots, 50$.

A perennial problem among those who interpret results from analysis of variance is that of determining which particular means or sets of means may be considered different from each other, having rejected the hypothesis of homogeneity for the total group of means under test. Recent approaches to the problem include those of Scheffé (84) and Paulson (70). Since the problem is one of general importance, we can expect additional clarification and further development in the near future.

Scheffé proposes a general method which serves to test among any or all linear combinations of means. He investigates the power of the test, and compares its efficiency with that of a similar method, earlier proposed by Tukey (98). For the case where each mean is based upon an equal number of observations, and where we are interested only in comparisons between pairs of means, to assess the hypothesis

$$\mu_i - \mu_j = 0,$$

for all $i \neq j$, Tukey's test is shown to be the more efficient. In general, when interest may reside in linear combinations of more than two means, Scheffé's test is more efficient. Both procedures depend upon similar logic. In each test, we compare the observed contrast

$$\hat{\theta} = \sum_{i=1}^k c_i \bar{x}_i, \text{ where } \sum_{i=1}^k c_i = 0,$$

with critical values for the contrast, each of which is equal to the product of an estimated standard error of the contrast and a value of the F -distribution (the Studentized range for Tukey's test) at the point α . These methods have a distinct advantage over the custom of making repeated t -tests, since the specified significance level, α , is for the proposed methods an over-all level which gives the probability that all decisions made about contrasts simultaneously are correct. With repeated, nonindependent t -tests, the nominal significance level is not the actual significance level, and the latter usually is not amenable to evaluation.

To exemplify Scheffé's method, consider the following situation, where from four statistically independent samples, each of equal size, N (specified only for convenience of this illustration), drawn from normally distributed populations with common variance, the hypothesis

$$H_0: \mu_1 = \mu_2 = \mu_3 = \mu_4$$

has been rejected from application of the usual analysis of variance test. It is now desired to determine whether the highest sample mean, say \bar{x}_2 , differs significantly from the three lowest values, say \bar{x}_1 , \bar{x}_3 , and \bar{x}_4 . For this problem, we compute $\hat{\theta}$ and $\hat{\sigma}_{\hat{\theta}}^2$ from

$$\begin{aligned}\hat{\theta}_{[1,3]} &= \bar{x}_2 - \frac{1}{3}(\bar{x}_1 + \bar{x}_3 + \bar{x}_4), \\ \hat{\sigma}_{\hat{\theta}}^2 &= \frac{\hat{\sigma}^2}{N} + \frac{1}{9} \left(\frac{\hat{\sigma}^2}{N} + \frac{\hat{\sigma}^2}{N} + \frac{\hat{\sigma}^2}{N} \right) = \frac{4}{3} \frac{\hat{\sigma}^2}{N},\end{aligned}$$

where $\hat{\sigma}^2$ is the best available estimate for the common population variance, providing only that $\nu \hat{\sigma}^2 / \sigma^2$ has the χ^2 distribution with ν degrees of freedom. In general,

$$\hat{\theta}_{[a,b]} = \frac{1}{a} \sum_{i=1}^a \bar{x}_{1i} - \frac{1}{b} \sum_{j=1}^b \bar{x}_{2j},$$

and

$$\hat{\sigma}_{\hat{\theta}}^2 = \left(\frac{1}{a} + \frac{1}{b} \right) \frac{\hat{\sigma}^2}{N},$$

where the contrast is between subset 1 and subset 2, with a and b means, respectively, in the subsets.

Define a positive constant S from

$$S^2 = (k-1)F(k-1, \nu),$$

where k is the total number of means included in a particular analysis of variance, ν is the number of degrees of freedom on which is based the estimate $\hat{\sigma}^2$, and $F(k-1, \nu)$ is the value from a table of F at probability α with $k-1$ and ν degrees of freedom. Then if

$$|\hat{\theta}| > S \hat{\sigma}_{\hat{\theta}},$$

the contrast $\hat{\theta}$ is significantly different from zero. The significance level, α , remains constant for a given analysis no matter how many of the possible

contrasts are being assessed, even for those comparisons suggested by the pattern of sample means. Of course, $\hat{\theta}$ and $\hat{\theta}_0$ depend upon a particular comparison under test.

Scheffé's work is extended by Roy & Bose (80), who generalize the approach to provide confidence intervals for a set of parameters such that the probability that all parameters in the set simultaneously are covered by the confidence intervals is a preassigned $1-\alpha$. The suggestions of Roy & Bose pertain to estimating or testing hypotheses not only concerning means, but also covariances and regression parameters from multivariate normal populations. The approach represented by Scheffé and by Roy & Bose provides a valuable solution for problems of simultaneous comparison of sets of statistics, since it yields a stable, known, over-all level of significance or of confidence.

Paulson (70) proposes a test for making a decision concerning whether or not a set of population means are homogeneous, and, if not, which is the best (highest). It is assumed that the same number of observations, N , is randomly selected from each of k normal populations with a common but unknown variance. The test yields one of $k+1$ decisions (that the k parametric means are homogeneous, or, that one of them is best), depending upon whether or not the value of the test statistic exceeds a critical value which depends only upon N , k , and α . An approximate formula for the critical value is provided in lieu of tables for exact values.

Study of the effect of unequal group variances on the F -test for means has continued. Horsnell (46) extends earlier work on this problem and verifies that, for moderate departure from homogeneity of variance (four groups, with combinations of population variances 1, 2, and 3) the effect is slight, so long as group sample sizes are equal. For the situation studied, the actual error in significance level is generally no greater than .01, when working either at the .05 or .01 level. While errors are relatively small for equal group sample sizes, much more serious errors may appear for unequal numbers in groups. For four groups, with variances of 1, 1, 1, and 3 and corresponding N 's of 12, 12, 12, and 4, instead of .05 the actual significance level is near .13, and instead of .01 the actual level is approximately .05. For unequal group sizes, errors are reduced if the size of the group exhibiting the largest variance exceeds that of the other groups.

The sensitivity of F to nonnormality, as well as to heterogeneity of variance, for equal sample sizes, has been studied extensively in a group of sampling investigations by D. W. Norton, reported by Lindquist (60). Norton selected more extreme variance differences than those studied by Horsnell, sampling from three populations with variances proportional to 1, 4, and 9. The study of three samples of 3 cases each yields for an apparent P of .05 an actual P near .07, and for an apparent P of .01 an actual P near .02. Decreased discrepancies of actual from apparent levels of significance appear when sample sizes are increased from 3 to 10. Even with rather extreme variance differences, then, the departure of the F -distribution from that

expected under the assumption of variance homogeneity is remarkably small, when sample sizes are equal. Norton's study demonstrates, further, that when selecting equal sized samples from populations the forms of which differ markedly either in skewness or kurtosis from the normal, the departure of the F -distribution from that expected under the assumption of normality generally is of an order no greater than that found for extreme heterogeneity of variance. The final phase of study involves four samples selected from extremely skewed, markedly skewed, moderately skewed, and normal populations with variances approximately 1, 8, 20, and 45. For samples of size 3, instead of .05, the actual level is .10, and instead of .01, the actual level is about .035.

It would be in error to conclude, on the basis of results from these and other sampling studies, that one can ignore the normality and equal variance assumptions in the F -test. There are a multitude of aberrant conditions, combinations of departures from normality and heterogeneous variances, which have not been subjected to scrutiny; existing results do not provide generalized conclusions to most such conditions. Further, even among conditions investigated, the differences between apparent and actual significance levels often are sufficiently large to be of practical importance in terms of the increased probability of erroneous rejection of the null hypothesis. Nevertheless, the remarkable insensitivity of the F -distribution to moderate departures from equality of variance and from normality, a finding confirmed over a number of studies, is reassuring for the usual experimental test situation, where information provided by samples is insufficient to provide a decision that the assumptions (for the population) are precisely met.

Gourlay (38) critically reviews and illustrates, in nonmathematical terms, the various uses of analysis of covariance for psychological research, giving special attention to the assumptions involved and to common misinterpretations of results. Abelson (1) suggests a computational procedure for analysis of covariance problems which incorporates advantages of the Neyman-Johnson technique (49), specifically the insensitivity of the latter to the covariance analysis assumption of a common coefficient of regression for all groups.

Estimation and tests of variances.—Current interest in nonparametric statistics has led to a number of proposals for the estimation of population measures of dispersion on the basis of sample ranges and similar sample statistics. One such proposal is of particular interest, since the statistic proposed has advantages even over the familiar estimate, s , of the population standard deviation. Keen, Page & Hartley (51) investigate, as an estimate of σ , the statistic

$$\hat{s} = \frac{\bar{w}\sqrt{\pi}}{2} = \frac{\bar{w}}{1.128},$$

where \bar{w} is the mean absolute difference between successive sample values,

$$w_1 = |x_2 - x_1|, w_2 = |x_3 - x_2|, \dots, w_{N-1} = |x_N - x_{N-1}|,$$

and

$$\bar{w} = \frac{\sum_{i=1}^{N-1} w_i}{N-1}.$$

This program regards each successive difference as a range from a random sample of two observations; each w_i thus is a range estimate for σ .

While the efficiency of this estimate is only .61 as compared with the ordinary estimate s in large random samples from a normal population, the statistic \bar{w} has the distinct advantage of remaining comparatively free from bias under conditions where variation is not random but follows systematic, gradually changing trends. Under such conditions, \bar{w} is often a much more precise estimate of variability about the trend line than is s . It should be noted that \bar{w} , unlike the estimate of σ based upon deviations from a trend (or regression) line, does not depend upon exact knowledge of the trend law underlying the data. Such a statistic can be of considerable value in estimating variability about a learning curve, or about successive stages of cyclic behavior.

Since homogeneity of variance is an assumption underlying the interpretation of results from analysis of variance on means, it has been the usual procedure to test for homogeneity of variance before investigating the homogeneity of a set of means. In view of this convention, a study of Box (9) is of considerable interest. Box investigates the sensitivity of Bartlett's familiar test for homogeneity of variance (7) to departures from normality. It is reported that Bartlett's test is so sensitive to nonnormality that it would serve quite well as a test for departure from zero kurtosis! The test becomes even more sensitive to kurtosis as the number of variance estimates under comparison increases. Such a result is in contrast to the many studies demonstrating that tests upon means (e.g., F , t) are relatively insensitive to departures from normality. (However, the study of Norton (60), discussed in an earlier section, suggests that the F -test is more sensitive to deviations from normal kurtosis than to departure from zero skewness.) Box suggests that for tests upon means, when little is known of the parent distribution, the practice of testing homogeneity of variance may well lead to more wrong decisions than if the preliminary tests were omitted. Further, there is increasing evidence that the analysis of variance test on means is not affected greatly by variance inequalities. Box comments, "To make the preliminary test on variances is rather like putting to sea in a rowing boat to find out whether conditions are sufficiently calm for an ocean liner to leave port." Since the F -statistic is so "robust," Box advocates its use in preference to nonparametric substitutes. When, with the usual F -test, it is suspected that departure from homogeneous variance is severe, or, with unequal subgroup sizes, that the variances are unequal, a recommended procedure is that given by Welch (101).

Box's criticism applies not only to the Bartlett test, but to all tests for homogeneity of variance which depend upon an expected variance under the assumption of normality. A procedure proposed by Box which would avoid the difficulty is an analysis of variance, after having split original groups into subgroups, on the means of logarithms of the subgroup variances.

For tests upon homogeneity of variances from samples which are correlated, neither Bartlett's test nor the F -ratio legitimately may be used. Kenny (53) discusses this issue, citing from statistical sources the appropriate test for the normal distribution two sample case. The statistic

$$t = \frac{(w-1)(N-2)}{\sqrt{4w(1-r^2)}}$$

has the Student t distribution with $N-2$ degrees of freedom, where N is the number of observations in each of the two samples, r is the product-moment correlation coefficient between the two sets of sample observations, and w is the ratio of the larger to the smaller sample sum of squares. Test procedure is similar to that for the F -ratio test of variances, where tabled probability values are doubled (for tests against a two-sided alternative hypothesis) to adjust for the arbitrary appearance of the larger estimate in the numerator of the statistic. While the use of t for testing the hypothesis of a common population variance is less sensitive than F when the correlation between two samples is zero or small, it remains the best approach for cases of appreciable correlation.

Truax (97) proposes a test on variances similar to that proposed for means by Paulson (70), which yields a decision that the variances are homogeneous or that a particular population demonstrates the greatest variance. For a two-sample nonparametric test of dispersion, tables are given by Rosenbaum (78). The test statistic is the number of observations from one sample which fall outside the range of a second sample. Critical values are listed for the .01 and .05 significance levels, for sample sizes from 2 to 50.

Measures of association.—Davidoff & Goheen (25) provide a single table for the estimation of the tetrachoric correlation coefficient, for all $p-q$ cuts. Given a four cell frequency diagram, the Davidoff-Goheen table is entered with the value ad/bc or its reciprocal, whichever is larger, where ad and bc are products of the diagonal cell entries. From the table is read a statistic, Q_3 , proposed by Pearson (71) as an estimate for r_{tet} . The authors compare both Q_3 and the estimate derived from the Chesire, Saffir & Thurstone computing diagrams (14) with the exact r_{tet} , for various representative values of $a+b$ and $a+c$. In evaluating these comparisons, one should note that, for the values presented, the average ($Q_3 - r_{tet}$) is .024, while the corresponding average difference for the estimate from complete computing diagrams is .001. Further, the average deviation of Q_3 about r_{tet} is .033, compared with a corresponding value of .019 for the computing diagram estimates. Since Q_3 seems to yield greater bias and less efficiency than the com-

puting diagrams, it would seem that the latter should remain generally preferred.

From a similar starting point, Hsü (48) presents a single chart for the determination of a Pearson estimate of r_{tet} , incorporating correction terms for p - q deviations from .5-.5. Unfortunately, as printed, the chart is practically unreadable, certainly not suitable for use.

Another approach to the estimation of tetrachoric coefficients is that of Michael, Hertzka & Perry (68) who present graphs for estimating r_{tet} , given the corresponding phi coefficient, for certain restricted p - q splits of particular interest in item analysis. The graphs may be used when one variable is split either .50-.50 or .27-.73. The derived values are estimates, correct to two digits (provided both the phi coefficient and p are correct to two places) for r_{tet} as computed with the aid of tables of Pearson (72).

A paper by Maritz (65) extends earlier work regarding an estimate of correlation in a bivariate normal population when one of the variables is dichotomized. The proposed statistic, G , serves as a substitute for biserial r , with the distinct advantage that, while r_{bis} is sensitive to any restriction upon the continuous variate, G is, in the cases studied, a more consistent estimate than r_{bis} of the population coefficient ρ , even when the assumptions underlying the interpretation of r_{bis} are met. Maritz also discusses computational procedures for the determination of G .

It is generally recognized that the contingency coefficient

$$C = \sqrt{\frac{\chi^2}{N + \chi^2}}$$

is not a satisfactory statistic for the measurement of strength of association in a contingency table. The possible range of the statistic has been shown to depend upon the order, $r \times s$, of the table; knowledge of the sampling distribution of C remains practically nil. For the case where both row and column entries in a contingency table are amenable to rank ordering, an association statistic, t_c , is suggested by Stuart (89); t_c is an estimate of the Kendall rank correlation coefficient, τ (52). An $r \times s$ contingency table, with a grand total of N observations, is regarded as two rankings of N objects, for one of which only r and for the other only s distinct ranks are recorded. The marginal totals then give simply the number of objects tied at each rank level. It is desired to find a statistic with possible range $-1 \leq t_c \leq 1$, even for large marginal totals. Such a statistic, an estimate of τ_c , is defined as

$$t_c = \frac{2S}{N^2} \frac{m}{m-1},$$

where m is the smaller of r and s , and S is determined by the rules which follow.

To determine S , first order columns from left to right and rows from top to bottom. Then, for each cell in the table, multiply its frequency (a) positively, by the frequencies of all cells lying below it and to its right;

(b) negatively, by the frequencies of all cells lying below it and to its left; ignoring all cells above it, in the same row, or below it in the same column. S is the sum of these products, taken over all cells in the table.

The procedure is illustrated by using fictitious data of Table I for which

TABLE I
CONTINGENCY TABLE

	High	Medium	Low	
College	50	45	5	100
High School	40	110	50	200
Grade School	10	145	45	200
	100	300	100	500

McNemar (63) has computed the coefficient of contingency, C . For this table, $m=3$, $N=500$, and $S=21,700$;

$$t_c = \frac{2(21,700)}{250,000} \cdot \frac{3}{2} = .260.$$

This value of t_c can be compared with the coefficient, $C=.39$, for the same data.

Stuart shows the expected value of t_c ,

$$E(t_c) = \frac{N-1}{N} \tau_c,$$

and an upper bound estimate of its sampling variance,

$$s^2_{t_c} \leq \frac{2}{N} \left[\left(\frac{m}{m-1} \right)^2 - t_c \right]^2.$$

The distribution of t_c is asymptotically normal, and a conservative confidence interval for the estimate (or a test on the significance of the difference of two estimates) is provided by the normal distribution with the use of the upper bound of $s^2_{t_c}$ as an estimate of sampling variance. For the present example, we find

$$s_{t_c} \leq \sqrt{\frac{2}{500} \left[\left(\frac{3}{2} \right)^2 - .260 \right]} = .126.$$

Using this upper bound s_{t_c} and the property of asymptotic normality, we may establish conservative .95 confidence limits about the estimate,

$$t_c \pm 1.96s_{t_c},$$

or

$$.007 \leq \tau_c \leq .513.$$

While t_c exhibits some advantages over C , it remains far from ideal. Its range easily is demonstrated to be restricted when marginal totals are unequal. The estimate of sampling variance seems extremely conservative. The statistic is only useful when interest resides in ordinal association between two sets of conditions. The greatest value of the approach accrues from its use to test differences between association in two contingency tables, for which it is the only technique available.

Problems of estimation of multiple regression parameters have been discussed by Perloff (73), Fraser (33), Wishart & Metakides (103), and by Durand (28), and are here reported by title only. Anderson (4) presents a useful review of literature pertaining to tests of significance of autocorrelation and discusses the problem of estimating regression coefficients, when errors of measurement are autocorrelated.

Many psychologists have found useful the tables of Finney (32), for an exact test of association in 2×2 contingency tables. Finney's tables provide critical values, at .05, .025, .01, and .005 significance levels, for contingency tables in which both frequencies in one of the margins are less than or equal to 15. These tables now have been extended by Latscha (56) up to marginal frequencies of 20. Another noteworthy table is that recently prepared by Lewis (59), providing values of chi square, correct to eight decimal places, at the .001 and .999 significance levels over a wide range of degrees of freedom.

Goodman (37) presents a valuable discussion of the Kolmogorov-Smirnov test for goodness of fit, with illustrations of the use of the test against both one-sided and two-sided alternatives for hypotheses (a) an observed sample has been randomly drawn from a known population, (b) two observed samples have been randomly drawn from the same (unknown) population. The test is more suitable than a chi square goodness of fit test, particularly for testing hypotheses of form (b).

General issues.—In brief notes, Marks (67) and Burke (11) discuss the propriety of one-tailed statistical tests. Marks emphasizes the need to consider the relative importance of Type I and Type II errors for particular hypotheses under test, and to select an optimum test in accordance with those considerations. Marks quite properly notes one-tailed analogues, for directional alternative hypotheses under tests of χ^2 and F . Burke proposes that there is no place for one-tailed tests in psychology. The argument is primarily a pragmatic one, that the practical consequence of the adoption of the one-tailed test will be the publication of a greater number of unreliable results. The view of the present writer is that safeguards against such practical consequences are "built in" to the statistical test model. Providing specification of the test model has been detailed in advance, the level of significance, α , serves to indicate the expected stability of results. If the same significance level is applied to the one-sided and the two-sided case, the same degree of reliability is indicated.

The problem of simultaneously assessing the significance of results from

several independent experiments is posed by Jones & Fiske (50). For testing the hypothesis that the composite P value for several findings exhibits only chance departure from .50, the writers discuss the statistic

$$\chi^2 = -2 \sum_{i=1}^k \log_e P_i,$$

with $2k$ degrees of freedom, where P_i is the P value associated with results from the i^{th} experiment. The P 's should be defined in accordance with the alternative hypothesis so that the possible range, $0 < P_i < 1.00$, never is curtailed. Sakoda, Cohen & Beall (81) provide graphs, based upon the binomial distribution, for quick determination of the probability of finding n results significant (at either the .01 or .05 level) from N independent experimental results. The graphs are useful in those cases for which N is not so large as to allow use of the normal approximation.

Hammond (42) elaborates arguments concerning the importance of randomization in experimental analysis and stresses restrictions upon generalizations of conclusions stemming from the nonrandom sampling of experimental conditions or effects. Presented are instructive examples of published reports where so little attention was paid the sampling of experimental effects that the investigators' generalizations of conclusions about effects are unjustified. Hammond (implicitly) gives justification for the separation of two analysis of variance models, the first for which experimental conditions may be assumed to have been randomly selected from a specified universe of conditions, the second for which the conditions or effects cannot be considered so sampled. For the first model, the population means for the conditions sampled are random variables, selected from the universe of effect means. Under this model, interaction variance appropriately may be used as the denominator of an F -ratio. Since the aim is to generalize concerning the existence of main effect differences in the population of effects, one wishes to discover whether main effect differences in the experiment remain significant beyond the expectation based upon interactions. Effect means are themselves random variables, so such interactions may be considered the result of fortuitous relations among the particular conditions sampled. Under the second model, conclusions are restricted to bear upon only those effects represented in the experiment. In this case significant interaction variance estimates are inappropriate error terms; under this model, the ratio which would result from the use of interaction terms in the denominator is not distributed as the familiar F -distribution.

Hammond's failure to distinguish the use of significant from insignificant interaction variance estimates as error terms leads to erroneous criticism of some earlier work. Of course, when interaction variance estimates (or even main effect estimates) are insignificant, these properly may be pooled for a more stable estimate of error, under either model. It is only to nonzero interactions (in the population) that the earlier remarks apply.

Of considerable value to researchers is the extensive bibliography of non-

parametric statistics by Savage (83). With an estimated 950 titles, a surprisingly large number considering the relative recency of extensive work in the area, Savage's comprehensive references cover a period including early 1953. The main bibliography contains listings alphabetically by author. A useful supplementary cross-reference gives papers under one of 17 topic headings, e.g., Contingency Tables, Correlation and Curve Fitting, Scaling, Applications, Tables, etc.

SURVEY SAMPLING

An extensive report of statistical problems in the work of Kinsey, Pomeroy, and Martin, *Sexual Behavior in the Human Male*, is presented by Cochran, Mosteller & Tukey (19). As an appendix to that critical report, the same authors have prepared a paper on principles of sampling (20) which, in nontechnical language, cites a number of practical sampling problems frequently encountered and presents general suggestions for efficient treatment of them. It is refreshing to find mathematical statisticians so aware of empirical reality as to devote sections to "the presence of systematic errors" and the "salvage of unplanned samples." Of considerable interest is the fluent discussion of subtle differences among classes of sampling plans for which, to assess stability of population estimates, appropriate formulas are different. The article also includes a worth-while discussion of when and how to adjust sample statistics for more precise estimates of parameters in the "target population."

Deming (26) pleads for a clear distinction between enumerative and analytic surveys, and for the recognition of differing formulas appropriate in the two cases. An enumerative survey is one for which the purpose is confined to estimating the frequency of a certain characteristic in the population; action is taken on the basis of the frequencies alone. An analytic survey has as its purpose the determination of causes of given frequency distributions, in order to make possible control over the frequencies in the future. The issues are not dissimilar from those raised by Hammond, discussed earlier; for the analytic survey there must be considered sampling from a universe of causes, as well as the more familiar sampling from a population of individuals. Appropriate estimates for variances of proportions are given and exemplified for several survey applications.

The numerous and complex decisions necessary for efficient survey planning are illustrated by Marks, Mauldin & Nisselson (66). There is emphasized the need for more precise knowledge of the nature of interview error; in particular, without a means for evaluating relative accuracy of alternative interview methods, there is no firm basis for decisions concerning interview procedures, decisions which properly should be a joint function of the cost and accuracy of those procedures.

PSYCHOMETRIC SCALING

A paper by Thurstone (95) deals with the present status of methods for "subjective" measurement. We find succinct restatements of the logic of

paired comparisons, the distinction between the contributions of Weber and Fechner to psychophysics, and the measurement requirements for prediction of choice. There are outlined numerous potential and actual applications of the law of comparative judgment, in fields of social psychology, esthetics, and experimental economics. In a second paper, Thurstone (94) outlines new methods currently under investigation. Many of these are closely related to the method of successive intervals, which exhibits telling practical advantages (and possible theoretical advantages) over the more laborious experimental procedures associated with paired comparisons.

For Case III of Thurstone's Law of Comparative Judgment (90), Burros & Gibson (12) have proposed a solution, based upon logic similar to that earlier proposed by Thurstone for Case IV (91). The solution for discriminial dispersions has the advantage over the earlier Thurstone approach that it is analytic rather than graphic; of course, the new solution does not depend on an assumption of similar magnitudes for the discriminial dispersions.

An alternative to Thurstone's model for paired comparison scaling, in some respects more flexible, is that developed by Bradley & Terry (10). Maximum likelihood estimates are derived for the scale value of each stimulus, the estimate depending upon observed paired comparison preferences. There are available tests on the stability of differences among stimulus ratings, as well as on the stability of subject to subject agreement. For small numbers of stimuli and subjects (or repeated judgments), tables have been prepared which serve to expedite application of the method. As the number of subjects increases, say for $N > 15$, approximate tests are possible on the basis of known asymptotic distribution properties. Bradley & Terry illustrate the methods using an example from a taste testing experiment.

Coombs, Raiffa & Thrall (22) discuss characteristics and uses of mathematical models, with particular reference to models of psychological measurement. The paper contains a clear discussion of the role of models in science and of the distinction between model and theory. For illustrative purposes, numerous distinct scaling models are characterized and differentiated; for 10 of these, assumptions are weaker than those implied by an equal interval scale. As models, there is little to criticize in these. As theories about data, however, one may wonder how fruitful (or how trivial) the distinctions among the various illustrative models will prove to be.

CLASSIFICATION AND TEST THEORY

Lord (62), beginning with the single assumption that a particular ability may be conceived as an ordered variable, represented numerically on a single dimension, deduces a wealth of conclusions concerning scores on a test of that ability. The deductions follow under rather general conditions if the items are homogeneous in the sense that the one ability is sufficient to "explain" all common variance and if the items cannot be correctly answered by guessing. Among the more interesting conclusions are: (a) if ability is considered to have an equal unit metric, raw test score units are in general unequal; (b) regression of test score on ability is in general curvilinear;

(c) the curvilinear correlation of test score and ability is always equal to the square root of the test reliability; (d) the distribution of errors of measurement is neither normal, nor, in general, symmetric; (e) the skewness of the distribution of errors, as well as the standard error of measurement vary with the ability level.

In a succinct paper by Horst (47), there is derived an estimate of test reliability, when dispersion of item difficulties is substantial. Consider the Loevinger coefficient of homogeneity (61),

$$H_t = \frac{\sigma^2_t - \sum p_i q_i}{\sigma^2_m - \sum p_i q_i},$$

where σ^2_t is the variance of test scores, p_i is the item difficulty, $q_i = 1 - p_i$, and σ^2_m is the maximum possible variance for a test with the distribution of item difficulties of the test under inspection,

$$\sigma^2_m = 2 \sum i p_i - M_t(1 + M_t).$$

M_t is the mean of test scores, and $\sum i p_i$ is the sum of products of p_i and the rank (1, 2, . . . , n) of the p_i . H_t is shown by Horst to be an estimate of average item intercorrelation, corrected for dispersion of item difficulty. If H_t is so considered, and is substituted for each r_{ii} in the Kuder-Richardson general formula 3 (54), the result can be reduced to

$$r_{tt} = \frac{\sigma^2_t - \sum p_i q_i \cdot \sigma^2_m}{\sigma^2_m - \sum p_i q_i \cdot \sigma^2_t}.$$

This equation generally yields a higher estimate of reliability, r_{tt} , than Kuder-Richardson formula 20, and also yields a higher value than Loevinger's H_t . The formula would seem to provide a more appropriate estimate of reliability than Kuder-Richardson 20, since it is not attenuated by dispersion of item difficulties. The Horst formula, like the Kuder-Richardson formulas, is a proper estimate of reliability only under the assumption "that all items measure the same function, and that failure of maximum item intercorrelation is due only to the unreliability of the items" (47).

Guttman (40) presents formal derivations of reliability coefficients for cases where errors of measurement for items need not be assumed independent. A formula for the lower bound reliability, estimated from a single experiment is

$$r_{tt} = \frac{n}{n-1} \cdot \left[1 - \frac{\sum \sigma^2_{x_i} + \delta}{\sigma^2_t} \right],$$

where n is the number of items (or subtests), $\sigma^2_{x_i}$ is the variance for the i^{th} item, σ^2_t is the general variance for total test scores, and δ is the sum of all interitem covariances of errors of unreliability, omitting self-variances. It might be noted that, where n is the number of items, $\sigma^2_{x_i} = p_i q_i$, and this formula reduces to Kuder-Richardson formula 20 less the factor

$$\frac{n}{n-1} \frac{\delta}{\sigma^2_i}$$

In order that the formula be useful, some assumption would be required concerning the nature of experimental dependence, in order that δ might be evaluated. A formula for the lower bound of a split-half correlation is derived from the above formula, substituting 2, the number of subtests, for n .

Dwyer (29) summarizes and systematizes the current status of the personnel classification problem, discusses it as a special case of the general problem of linear programming, then proposes a method of solution for it as a special case. The method is not dissimilar from, but appears somewhat more general than, solutions proposed earlier by others.

FACTOR ANALYSIS

Three crucial problems in factor analysis, to which solutions have seemed most elusive, are when to stop factoring, how to determine the statistical stability of factor loadings, and how to rotate axes objectively to achieve unique simple structure. Important advances have been made toward solution of all three during the year.

Assuming a joint multivariate normal distribution of variates, Rippe (76) develops a statistical test for the number of significant common factors, a test which is applicable to any method of factoring, whether it begins with the sample correlation matrix or the sample covariance matrix. Rippe's solution is comparable to that developed by Lawley (57), but because it is derived from distribution functions (notably the Wishart distribution of sample covariances) known to obtain whenever variates are sampled from a multivariate normal distribution, it is not restricted to particular types of factor solutions.

Rippe develops the conclusion that a statistic λ is distributed as χ^2 with $\frac{1}{2}(k-s)(k-s+1)$ degrees of freedom, where k is the number of variables, s is the (trial) number of factors, and

$$\lambda = (N-1)(\log |\mu_{ij}| - \log |m_{ij}| + \sum \mu^{ij} m_{ij} - k).$$

N is the sample size; $|m_{ij}|$ is the determinant of the matrix formed by elements

$$\mu_{ij} = \sum_{p=1}^s a_{ip} a_{jp} + \delta_{ij} a_i a_i,$$

where a_{ip} is the loading for the p^{th} factor on the i^{th} variable, a_i is the unique component loading for the i^{th} variable, and δ_{ij} is Kronecker delta; $|m_{ij}|$ is the determinant of the original correlation (or covariance) matrix with elements m_{ij} , and μ^{ij} is the ij^{th} element of the inverse of (μ_{ij}) .

After the extraction of s factors, λ is compared with the critical value of χ^2 at a prearranged level of significance. The hypothesis tested is that

(m_{ij}) may be regarded as the covariance matrix for a random sample drawn from a population with covariance matrix (μ_{ij}). If $\lambda = \chi^2$ is so large as to be significant, the hypothesis is rejected, and another factor is extracted. If λ is less than the critical value of χ^2 , only s significant factors are required.

The major disadvantage of Rippe's procedure is the amount of labor required, which for large k "may be considerable." Rippe does refer to short-cut procedures for computing $|\mu_{ij}|$ and (μ^{ij}), which, for s much smaller than k (as is typically the case) make the procedure feasible. It should be noted that while the procedure does lend confidence in the appropriate number of factors for a given correlation matrix, common factor procedure will continue to depend upon the precision of estimates for communality, or uniqueness.

Rippe further extends the usefulness of the test procedure by developing tests for the statistical significance of individual factor solutions. Happily, the additional computations required for tests on particular factor loadings are relatively slight, presuming $|\mu_{ij}|$ and (μ^{ij}) already have been found. Both the test on the number of factors and the test for stability of factor loadings are illustrated by Rippe, with a 13 variable, 3 factor example.

Thurstone (93) has proposed an analytic method for rotation to simple structure, which depends upon the application of linear weights for each variable, with weights inversely proportional to the projection of the variable on a trial vector, P . If v_{jp} is the scalar product of test vector J and trial reference vector P , it is desired to adjust the position of P so as to minimize the criterion function

$$\phi_p = \sum_j w_{jp} v_{jp}^2.$$

The weights, w_{jp} , depend inversely upon v_{jp} . Minimization of ϕ_p yields a final P vector normal to and defining a reference plane.

In an empirical investigation of Thurstone's proposed analytic method for rotation, Bargmann (6) reports results from a 21 variable, 8 factor study of psychological data. The method yields an excellent unique simple structure. In a second analysis of the same data, Bargmann combined the linear weighting procedure suggested by Thurstone with a successive approximation "single planing" method of rotation (92). Again, the structure was very satisfactory, and for this innovation the total work time was reduced by about one-third. Each application, the analytic method as proposed and the combination with single planing, very substantially reduced the amount of time from that required to achieve satisfactory oblique graphical solution. More importantly, the new methods do allow for objective factor solutions, unaffected either by biases of the analyst or by his inability to achieve optimal graphical solution.

Saunders (82) presents still another analytic method for rotation to simple structure. Each factor loading is tallied with both its ordinary and reflected sign and for every factor a hyperplane is determined so that this "doubled" distribution of factor loadings about the hyperplane exhibits maximum kurtosis. The method as presented applies only to orthogonal

solutions, and for such cases results are reported to be numerically identical to those achieved from application of the method earlier proposed by Carroll (13).

So little empirical investigation has been reported regarding the several proposed analytic methods of rotation that their relative merits are difficult to evaluate. It is judged, however, that Thurstone's method will prove the most generally adaptable of those proposed, and that solutions obtained from it will most nearly correspond to the corresponding solution from a laborious, competent graphical rotational procedure for cases in which simple structure is overdetermined.

A promising avenue for investigation of the statistical stability of simple structure is that of Bargmann (5). By ingenious use of the geometrical model for simple structure and the binomial probability distribution, Bargmann has devised a test to determine the number of zero loadings (entries within a specified range of zero) which are required for the reliable location of a hyperplane. Bargmann indicates that we may expect more detailed reports of this work in the near future.

Briefly noted, other recent important contributions related to factor analysis include: Guttman's (41) description of image analysis, an application of multiple correlation theory to the study of common and unique components of observed variates; the suggestions of Wherry & Winer (102) concerning a variant of a multiple group method by which factor loadings may be estimated without first having computed the matrix of intercorrelations among the variables; Harmon's (45) discussion of multiple group methods with special emphasis on the computational usefulness of the square root method for simultaneous solution of a set of linear equations; Vincent's (99) historical introductory exposition of factor analysis; and the work of Ahmavaara (3) reporting formal proof of several theorems regarding effects of selection of populations upon invariance of simple structure and of factor loadings.

The over-all picture in factor analysis is one of considerable progress, with the clarification of a number of most troublesome issues. In the foreseeable future, factor analysis may be expected to graduate from a technique suspected by many to one respected by most. The further refinement and general adoption of procedures recently available can result only in increasing objectivity and power of factor analysis as a multivariate technique.

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EDUCATIONAL PSYCHOLOGY¹

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INTRODUCTION

Subject matter of educational psychology.—Educational psychology is an empirical, or scientific, approach to education which attempts to determine, select, and apply to educational practice relevant generalizations regarding the observable behavior of the organism, especially behavior that is subject to change with experience and behavior involving social situations, in order to (a) develop in the pupil or student an understanding of the physical, biological, and social worlds in which he lives and (b) enhance his chances of adjustment to these worlds.

This is an inescapably broad definition encompassing a great deal of content and implying interdisciplinary interests and contributions. The wide range of subject matter which comprises educational psychology is illustrated by the chapter headings of general textbooks in the field, or by the section headings of the present and earlier research summaries appearing in the *Annual Review of Psychology*.

Within the broad field of educational psychology the shifting interests and biases of instructors and researchers lead to varying degrees of emphasis on the several content areas from time to time. Over the last 25 years the reviewer believed he could discern a number of trends indicative of such changing emphases. For example, trends appeared to be in progress toward greater interest in growth processes, toward more concern over emotional and social development, toward more attention to the gifted child, toward greater emphasis upon the "clinical approach" and adjustment problems, toward somewhat more care to research design in educational investigations, and toward a merging of social psychology and educational psychology at many points.

In an effort to determine whether or not these impressions had any foundation, the reviewer undertook a cursory check of topics treated in three general textbooks of educational psychology widely used during the period 1925 to 1930, in two general educational psychology textbooks that became available early in 1954, in issues of the *Journal of Educational Psychology*

¹ This review covers approximately the 12 month period ending in April, 1954. Space limitations permit reference to only a fraction of the numerous publications that appeared; those selected for inclusion represent, in the reviewer's opinion, a fair sample, but many equally important discussions have been omitted. The reader is reminded that other chapters of the *Annual Review of Psychology* also pertain to aspects of educational psychology (e.g., Child Psychology, Learning, Individual Differences, Theory and Techniques of Assessment, Counseling, Statistics, etc.) and that reference to such chapters will provide more complete coverage of the recent literature in the field.

published in 1930, and in 1953-54 issues of the same journal. The sampling obviously is very restricted, but the data shown in Table I are at least suggestive.

The table indicates, in approximate percentages, the proportionate number of pages devoted to each of several topics by contemporary textbooks and textbooks of a quarter of a century ago, and the proportionate number of articles, dealing with the same topics, carried by the *Journal of Educational Psychology* in 1953-54 and in 1930.

TABLE I
COMPARISON* OF TOPICS TREATED IN SELECTED EDUCATIONAL PSYCHOLOGY
TEXTBOOKS AND IN THE JOURNAL OF EDUCATIONAL PSYCHOLOGY
CIRCA 1925 TO 1930 AND 1954

Topic	Three Widely Used General Textbooks 1925-30 (Per cent of Total Pages)	Two General Textbooks 1954 (Per cent of Total Pages)	<i>Journal of Educational Psychology</i> 1930 1953-54 (Per cent of Total Articles)	
Physiological and Psychological Bases of Behavior	29	5	10	4
Individual Differences (including ex- ceptional children)	9	8	17	21
Psychology of Learning	34	{ 19	8	6
Learning of School Subjects	9		14	23
Motivation	2	10	0	1
Personal-Social Characteristics of Children	1	12	5	12
Adjustment Problems and Mental Hygiene	2	13	1	10
Growth, Development, and "Readi- ness"	2	8	0	2
Professional and Personal Consid- erations of the Teacher	0	3	0	2
Measurement (including statistics), Assessment, Evaluation, and Guidance	8	18	45	19
Other	4	4		

* The classification obviously is arbitrary and reflects the reviewer's judgment. Percentages refer to approximations of the proportionate (a) number of pages devoted to each topic by textbooks and (b) number of articles dealing with the topic carried by the *Journal of Educational Psychology*.

With regard to the textbooks of educational psychology, there was a notable decrease in amount of space given to the review of psychological and physiological topics ordinarily covered in a general psychology course.

There also was a decrease in content devoted to the psychology of learning, particularly more abstract principles. On the other hand such topics as the personal-social characteristics of children, adjustment problems and mental hygiene, growth and development, and measurement, evaluation, and guidance were accorded more attention in 1954 compared with the earlier period.

In issues of the *Journal of Educational Psychology* the comparative data indicate some decrease over the years in articles dealing with physiological and psychological bases of behavior and a pronounced reduction in papers having to do with measurement (including statistics), evaluation, and guidance. The proportionate number of articles dealing with the other topics noted in Table I shows some increase in 1953-54 as compared with 1930.

General treatments of educational psychology.—Several new textbooks of educational psychology appeared during the period of the review. Cronbach's *Educational Psychology* (1) is particularly noteworthy and probably will become one of the popular contemporary texts. The treatment is thoroughly rigorous and accurate, and at the same time the materials have been clearly written and provided with study-aids that should be particularly helpful to the student. The presentation is objective, and sources and evidences refer largely to empirical studies. The principles presented are tied closely to classroom problems and classroom situations.

The viewpoint taken by Cronbach is that socialization of the individual is a major objective of education, the aim of socialization being the development of an individual who takes care of his physical security, wants to do what society considers good (at the same time, being critical of conventional values), solves problems as correctly as the available facts permit, has self-respect and self-confidence, is effective in dealing with other people (and respects the rights of others, and helps them), and has some absorbing personal goals, interests, and sources of satisfaction.

As would be expected a major portion of the book is devoted to the problem of learning, with attention to the concepts of: (a) goal, some consequence the learner wishes to obtain; (b) readiness, response patterns and abilities the learner possesses at any time; (c) situation, the objects, persons, and symbols in the learner's environment; (d) interpretation, the process of directing attention to parts of the situation, relating them to past experiences, and predicting what can be expected to happen if various responses are made; (e) response, either an action or an internal change that prepares the learner for action; (f) consequence, events that follow a response that are regarded as confirming or contradicting it; and (g) reaction to thwarting, behavior that occurs when a person fails to attain his goal.

Two other recently published textbooks in general educational psychology will be of interest to certain instructors. That of Smith (2), which appears under the title, *Psychology in Teaching*, and one by Blair, Jones & Simpson (3) are somewhat more conventional in approach than the Cronbach volume. Both books make an effort to be practical, to deal with classroom situations, and to interject classroom examples. A unique feature of

the Blair, Jones & Simpson book is a section dealing with professional problems of the teacher and the personal and emotional adjustment of the teacher.

A less orthodox treatment, apparently intended as a textbook in educational psychology, is Cantor's *The Teaching-Learning Process* (4). The book is "different" both with regard to the selection of subject matter and to method of presentation. A unique form of presentation is used in that principles are stated and then reinforcement is attempted through the use of transcripts from discussions of the principles carried on by groups of experienced teachers. The main thesis of this book is that pupils and teachers interact and affect each other, and that effective educational processes are dependent upon high morale and close rapport of pupils and teachers, contributed to by democratic classroom activities and by insights from clinical psychology. The author is thoroughgoing in his adherence to a central thread built around group behavior and group therapy. Even with the apparent trend toward the application of certain clinical principles to the understanding of the average school child, it is doubtful that any large group of educational psychologists or teachers will accept the extreme viewpoint set forth in this book.

SCHOOL LEARNING

Learning in school subjects.—As was true of most topics covered by this review, the number of studies reported in the area of classroom learning was large. Many, however, had to do with isolated or highly specific aspects of learning and are not included here.

The matter of achievement in present day schools is one that recently has caused both educators and citizenry concern. Burke & Anderson (5) sought an answer to the question of the relative achievement of elementary pupils attending school in 1939 and in 1950 in one small community. Application of analysis of variance, covariance, and Cochran-Cox techniques to Metropolitan Achievement Tests data showed a majority of the obtained statistically significant differences favoring the 1939 pupils. It was pointed out, however, that factors such as teacher experience, teacher competence, kindergarten experience of some pupils, and various environmental factors could not be adequately controlled. More investigations of this kind should help to resolve current debate and to provide an empirical basis for thinking about the curriculum.

Two pamphlets, *Teaching Arithmetic* (6) and *Teaching Spelling* (7) were published early in 1954 by the National Education Association in an avowed effort to aid the typical classroom teacher in keeping pace with new research findings. The materials are treated relatively informally and very concisely. There is no documentation. The arithmetic booklet seems to be a complete, brief presentation of problems encountered in teaching arithmetic and how such problems may be approached by the teacher. The spelling pamphlet similarly is straightforward and covers various instructional principles. The

relationship of the materials to specific researches, or even to the "Selected Research References" provided, is not readily apparent, and it is doubtful that any research basis can be found for some principles and suggestions presented. However, teachers should find the pamphlets useful as practical teaching aids.

Several investigators have concerned themselves with methods of teaching the language arts and skills. Jackson (8) reported that an experimental group using word analysis in learning to spell did not achieve significantly better than a control group, which used only the textbook. In another study at approximately the same grade level Templin (9) constructed phonic tests and administered them, together with spelling and reading tests, to a large group of children just completing grade four. High and low scores on the spelling and reading tests were compared and were sharply distinguished by the phonics tests results. It is possible that had Jackson controlled phonics knowledge, he might have found the word analysis method more effective with children who had poor phonic knowledge at the beginning of the experiment.

In the area of reading there has been proper concern over remedial techniques. Coleman (10) studied 40 male reading disability cases of average or above-average intelligence, with attention to an hypothesized relationship between reading disability and perceptual retardation. The hypothesis appeared to be confirmed. Coleman discusses the possible value of perceptual differentiation training as a symptomatic treatment for reading disability. French (11) provided empirical support for the view that kinesthetics recognition may be related to learning to read. Results of a kinesthetics recognition test administered to groups of retarded and nonretarded oral readers, matched for chronological age, mental age, and IQ, showed a statistically significant difference favoring the nonretarded readers.

Harris & Roswell (12) reviewed the problem of reading difficulties and their probable causal factors. Available diagnostic methods were described and a multiple-procedure approach recommended.

Maize (13) compared two methods of teaching English composition to retarded college freshmen. Students who had attained low scores on the American Council on Education Psychological Examination and the Purdue Placement Test in English, and who also had received a below-passing grade on an original theme, were divided into control and experimental groups of 74 and 75 members respectively. The control group was taught by a grammar workbook-drill method, whereas the experimental group was taught by a writing-laboratory method. Both groups showed significant changes in vocabulary, mechanics of English, and grammar, but the experimental group also showed significant gain in composition and a significantly larger change, as compared with the control, in all of the areas measured with the exception of vocabulary. The efficacy of active participation in the skill was supported.

In the field of arithmetic Brownell (14) investigated the effect of practice of a more complex skill upon proficiency at its constituent subskills, noting

that practice at the complex intervening skill showed no uniform effect in changing the subskills. There was some evidence that the oldest established subskill (subtraction) was more resistant to either improvement or deterioration than subskills more recently taught, while the greatest change was found to take place in the subskill (simple division) most like the complex skill practice.

Conditions of learning.—Various conditions of classroom learning have been studied. Kephart & Floyd (15) compared randomly selected groups of fourth and fifth grade pupils who were assigned to experimental and control rooms, the experimental room differing from the control room in that the distribution of light was improved, lighter floor and wall colors were used to reduce contrast, and desks were arranged in curved rows to minimize undesirable shadows. Observations made at the end of a year seemed to indicate that students assigned to the experimental rooms were superior to controls in school achievement, posture, and freedom from extraneous movements or nervous habits. It should be noted that such improvement frequently has been observed in experimental groups where the members were conscious of their participation, and had an experimental design employing additional control groups been followed this possible effect might have been taken into account.

Adams (16) reported that fifth grade children enrolled in 19 combination "fourth-fifth grade" classes tended to achieve as well in skills measured as did pupils in regular fifth grade classes and concluded that the data gave no support to the hypothesis that children are retarded in their achievement by being grouped with pupils of a lower grade level.

Two groups of college students, one made up of individuals who studied for an "adolescent development" course independently and the other consisting of regularly attending students, were compared by Jensen (17) with respect to several factors. The groups achieved equally well when application of information was measured, although the regularly attending group scored higher on tests of knowledge of facts and principles.

Auble & Mech (18) reported an attempt to test in third grade classrooms a concept derived from learning theory, specifically, the tendency toward decrease in response strength with increase in delay of reinforcement. Covariance analysis of both error and correct responses in learning one-digit arithmetic combinations showed no significant differences that could be attributed to the delay of reinforcement.

Mech *et al.* (19) studied the relative effects of 100 per cent, 50 per cent, and 0 per cent verbal reinforcement on the learning of fourth grade pupils under conditions of massed training-massed extinction, massed training-spaced extinction, spaced training-massed extinction, and spaced training-spaced extinction, concluding that massed training with 100 per cent reinforcement was most satisfactory.

Of importance from the standpoint of learning theory and method is the new volume by McClelland *et al.* (20) summarizing researches and conclu-

sions relative to the development of methods for measuring aspects of the "achievement-motive." The methodology, which has certain implications for educational research, is concerned with procedures for producing ego-involvement and experimentally introduced experiences of success and failure, and with the estimation of the effects of such conditions from subjects' fantasies.

It is not uncommon to assume a positive relationship between morale and success at a task. Anderson (21) investigated this hypothesis in the field of teaching. Analysis of variance of the data suggested a significant difference between the average teacher morale scores in schools ranking high in student achievement as compared with those in schools ranking low, teachers with relatively high morale being found more frequently in schools where the students achieved more successfully. One interpretation would be that teachers with high morale are more effective teachers. It is possible, of course, that uncontrolled factors may account for the relationship.

Ash & Carlton (22) studied the learning from instructional films by equated groups of college freshmen under conditions of note taking during showing of the film and "no note taking," concluding that "film only" yielded the best results, probably as a result of the divided attention required by note taking during the showing.

INDIVIDUAL DEVELOPMENT

General treatments.—Two recent issues of the *Review of Educational Research* (23, 24) were devoted to growth and development in relation to the educational program during childhood and adolescence. Hurlock (25) brought together in one volume, under the title *Developmental Psychology*, discussions of behavior during four periods of life: childhood, adolescence, adulthood, and old age. Seidman *et al.* (26) published a comprehensive collection of readings on adolescent behavior, with materials drawn from education, psychology, sociology, biology, and anthropology.

Theory and measurement.—Child psychologists recently have given considerable attention to the comparability of development in different mental and physical aspects and have suggested the use of such concepts as "growth age" and "organismic age." Tyler (27) criticizes (a) the concept of growth age as a unit for measuring growth in both structures and functions, and (b) the averaging of growth ages for such diverse characteristics as height, dentition, intelligence, and education to obtain an "organismic age." The validity of the concepts is questioned from the standpoints of interpretation of the basic data, statistical justification, psychological meaning, and educational implications. It is suggested that analyses of longitudinal data be made to test specific hypotheses relating to the concepts of growth age, organismic age, and patterns of growth before such viewpoints are promulgated and promoted.

With concern for measurement methods, Bell (28) suggests a "convergence" approach to developmental data. Convergence techniques involve the

cross-sectional study of several different age groups, the age groups being spaced so that repeated measurement over a given period of time (longitudinal study) yields measurements at overlapping ages, providing information about changes occurring from the age of the first measurement of the lowest age group to the age of the last measurement of the highest age group. The technique should be useful for cross-sectional studies where only limited measurement is possible and the comparability of experimental groups on relevant factors other than age is questionable. Similarly, it should be useful for longitudinal studies where there is possibility that frequently repeated measurement may contaminate the experimental groups or where members of experimental groups are transient or unco-operative. The technique is not applicable to studies of the cumulative effect of an independent variable on a single individual, but it can be used in following statistically meaningful subgroups of an experimental group.

Physical and mental growth.—Nicolson & Hanley (29) obtained various measures of physiological and anatomical maturity on members of an urban sample of boys and girls, with repeated annual observations from the first through the eighth year and semiannual observations thereafter until the eighteenth year. Factor analysis of intercorrelations between the obtained indices was undertaken and a general factor reported. "Ninety per cent of mature height" appeared to be the best single measure of physiological maturity, but since such a measure cannot be available until growth in height is complete, indices relating to sexual development stages and skeletal ages seem to be satisfactory substitutes.

Ausubel & Schiff (30) used a teeter-totter problem to introduce relevant and irrelevant causal relationships to samples of 60 each of kindergarten, third grade, and sixth grade children, and concluded the ability to apply a relevant causal sequence and to disregard an irrelevant causal sequence increases with age, although even kindergarten children are able to learn irrelevant relationships as readily as relevant relationships.

Corsini & Fassett (31) criticized certain investigations of aging and accompanying change in intelligence, claiming inadequate sampling and the use of inappropriate measuring instruments. They reported the testing of an adult sample with the Wechsler-Bellevue Test from which they concluded there is no steady decline in general intelligence from early to late maturity, but that test ability declines if the subtests contain visual and motor components and increases for subtests involving materials that depend on continued learning.

Owens (32) reported that readministration of the Army Alpha Examination to a group of men after a 30 year interval showed an increase in mean total score and also an increase in individual differences among members of the group.

Adjustment.—Schrupp & Gjerde (33), repeating Wickman's 1927 study of teachers' attitudes toward child behavior, obtained from 39 mental hy-

gienists and 119 elementary and secondary school teachers' expressions of their attitudes regarding the seriousness of children's behavior problems. Teachers and mental hygienists were more in agreement in the 1951 sample as compared with the results of the 1927 study. Differences between attitudes of teachers and clinicians, however, still were evident and these disagreements were similar to those pointed out by Wickman. Clarke (34) reported that teachers whose adjustment indices were low expressed attitudes toward pupil behavior problems that were more in accord with the mental hygiene viewpoint than did teachers with high adjustment scores.

Bedoian (35) studied mental health data of under-age, at-age, and over-age sixth grade pupils reporting that under-age and at-age pupils attained significantly higher mental health scores than over-age pupils. Mental health also appeared to be related to social acceptance in that over-age groups enjoying superior sociometric status attained average and above-average mental health scores.

PERSONAL-SOCIAL BEHAVIOR

Interpersonal relations.—Loban (36) administered a variation of the "Guess Who" procedure to adolescents in grades 8 to 12 in nine different schools, assigning a score to each individual according to frequency of mention with respect to items describing sensitive, sympathetic personalities and items describing insensitive, inconsiderate personalities. The same subjects also were rated on a social sensitivity scale by their teachers. The 60 most and the 60 least socially sensitive children were compared on a number of variables. There appeared to be little difference between the high and low groups with regard to size of family, birth order, reading ability, intelligence, church attendance, or religious denomination. The most socially sensitive adolescents did seem to be more concerned over their relations with other people, to be more aware of their own limitations and inadequacies, to show greater interest in matters dealing with idealistic and sympathetic themes, to be more emotionally stable, and to consider themselves in better health, compared with the least socially sensitive. Bonney & Powell (37) studying small groups of first grade children found some evidence that sociometrically high individuals tended to be more conforming, to smile more frequently, to be more co-operative, and to make more voluntary contributions to the group. Gage (38) reported that study of social perception in relation to effectiveness in interpersonal relations suggests a positive correlation, and that accuracy of social perception appears to involve two negatively related abilities, one having to do with accuracy in perceiving manifest stimulus value and the other relating to accuracy at assuming the role of the person perceived.

Gronlund (39) found that teachers tended to prefer those pupils whose sociometric ratings were high, which suggests that factors influencing their opinions were similar to those affecting the opinions of the pupils' peers.

Keislar (40) found no relationship between peer ratings and "Getting along with others" scores of a self-report inventory. Scandrette (41), on the other hand, reported frequency of choice as a co-worker on a social science project committee to be related to inventory scores having to do with school relations, sense of personal worth, sense of personal freedom, feeling of belonging, and freedom from withdrawing tendencies. Results of the Keislar and Scandrette studies are not, of course, necessarily antithetical in light of variations in sampling of subjects, inventory content, and conditions of administration.

The validity of choice of friends as a method of measuring social adjustment has been questioned by Fox & Segal (42). Analyses of data lead the authors to suggest that choice-type social ratings have little or no relation to the individual's actual social adjustment. It is maintained, further, that children and youth tend to choose friends having the same traits that they themselves have; also, that they tend to select traits which may be classified as extrovert or socially aggressive, whereas extroversion and social aggression are not necessarily representative of desirable social behavior or good social adjustment.

Social groups.—Increasing attention to problems of human relations, and to group behavior and its expression in the school, was recognized by the *Review of Educational Research* (43) with an issue dealing with those topics. Roseborough (44) dealt with related considerations, reviewing experimental studies of small groups, from the standpoints of: group versus individual performance; lecture methods versus group discussion methods; attitude change with diffuse authority relationships; group affiliations; situational variables such as task problem, size of group, spatial position, and communicative pattern; and personality variables, particularly those relating to leadership behavior. It was pointed out that promising research techniques are available, and they have been used to investigate a variety of substantive problems.

Watson (45) attempted an evaluation of the work of small groups in a large class; he found little relationship between benefits from small group participation and the following: students' expressed preferences for group work, their levels of mastery of course materials, their stated interests to learn about "group leadership," their general level of enthusiasm for course topics, and their responses to questions relating to sympathy, hostility, self-reliance, etc. The most effective groups appeared to be larger than average, having 10 to 15 members each.

Keislar (46) considered the hypothesis that prestige social groups or cliques are often regarded as a divisive influence in the high school because of personal antagonisms they are presumed to foster. However, no evidence was found to indicate that girls who did not belong to prestige groups (Y clubs) held antagonism toward, or viewed in a socially unfavorable light, girls who did belong. On traits related to social acceptability nonmembers rated members of the clubs higher than they rated themselves.

EXCEPTIONAL CHILDREN

General treatments.—Revisions of two widely used textbooks, Baker's *Introduction to Exceptional Children* (47) and Heck's *The Education of Exceptional Children* (48), have been published during the year. Coverage of topics is roughly similar in the two volumes, almost half of each being devoted to physical handicaps and proportionately less to social and mental considerations. Much of the Heck book is devoted to matters of special classes, special schools, and problems of teaching the handicapped. Research relative to the education of exceptional children as recorded in the literature during the past decade was reviewed in a recent issue of the *Review of Educational Research* (49) under such headings as the following: general problems and administration of programs; the mentally handicapped; the gifted; the socially maladjusted; the auditorially and speech handicapped; the visually handicapped; and the orthopedically handicapped.

Delinquency.—Hathaway & Monachesi (50) reported on a number of studies concerned with the relation between the Minnesota Multiphasic Personality Inventory scores and subsequent evidences of delinquency. The psychopathic deviate and hypomania scales appeared to have value as predictors of delinquency.

The gifted.—An increasing amount of discussion has been focused on the gifted child, although researches in the area still appear to be relatively few. A special report prepared by a subcommittee of the American Psychological Association's Division of School Psychologists (51) suggested areas of needed research relative to giftedness. For want of a better criterion, giftedness was defined in terms of test level, the gifted child being one "whose rate of mental growth is 1.4 or 1.5 mental years per calendar year or faster." No experimental designs were proposed by the committee, but general questions, such as the following, were posed, each of which might encompass a family of research projects: What are the relative emotional, social, and intellectual effects of (a) keeping the child with his chronological age group and "enriching the curriculum," (b) locating the individual close to his mental age level in school class with chronologically older children, and (c) establishing special classes for children who have high rates of mental growth? To what extent is there "concealed failure" among gifted children (i.e., are they operating below their appropriate achievement level although not failing by the school standards)? What effects upon a gifted child result from various characteristics of his teacher? What are the effects of beginning vocational planning and vocational preparation at various times in the life of the gifted child? Are there special frustrations that impinge upon the gifted child that less often affect other children?

Wilson (52) administered various special ability tests to a group of 39 boys and 49 girls with high IQ's. The gifted children were found to be superior also in abilities related to art judgment, musical memory, science, and mechanical abilities. Correlations between special ability test scores and Wechsler-Bellevue results were generally positive and moderate.

EDUCATIONAL GUIDANCE

General treatments.—The field of educational guidance apparently has been undergoing a gradual change in the direction of clinical psychology, with increasing interest in the analysis of human motives and the stimulation of "dynamic mechanisms" in the individual as trends. Such treatments as those presented in an issue of the *Review of Educational Research* (53) dealing with guidance, counseling, and pupil personnel and in Humphreys & Traxler, *Guidance Services* (54), illustrate the expanding subject matter in this area.

Gage (55) makes a strong case for the desirability of counselors possessing a high degree of social perception or empathy. A number of related studies having to do with the perception of attitudes and opinions of other persons are reviewed, and the relationship between social perception and effectiveness as a teacher, as a supervisor of a clerical staff, or as a group leader is indicated.

The case histories of 27 typical high school boys and girls, randomly selected from a counseled group of 364 students, were presented in considerable completeness by Rothney (56). The collection of cases is unique in that attention is focused on representative students rather than on deviates. The materials should be very useful in the training of counselors and guidance workers.

Prediction and predictors.—Bingham (57) urged the development of expectancy tables, indicating for any individual whose score on a predictor lies within a certain range the probable level of success in the criterion behavior against which the predictor was validated. Such expectancy tables are employed relatively infrequently by educational counselors, but properly used they could be of great value in making practical suggestions to students. Although the evasive and perplexing criterion problem has presented a formidable difficulty, considerably more could be accomplished in the development of "expectancies" than has been done.

Flanagan (58) introduced a new battery of aptitude test materials employing the "job element" approach, which is described as intermediate between the "miniature job sample" and the "primary mental factors" approaches to differential prediction. The battery was developed after analysis of critical behaviors involved in various jobs and their classification into job elements. Fourteen "job elements" tests were devised and studied before introduction for use. Reliability coefficients range from .55 to .86. Intercorrelations are positive, but relatively low. Correlation coefficients suggesting the validity of different combinations of the tests for various occupations are based upon testing prior to entrance upon an occupation in relation to later job success. Multiple correlations with occupational and college success range from .46 to .89.

Multivariate statistical tools are beginning to be used in the analysis and prediction of group differences. Christensen (59) studied measures of college ability with five different groupings of college students, obtaining significant

D^2 values (the generalized distance statistic) for all but one pairing of the groups. Application of linear discrimination functions to a new sample resulted in classification significantly better than chance.

Tyler (60) used similar techniques in an attempt to predict student teaching success from personality inventory data. Problems of pattern analysis, and the related topic of profile analysis, have received increasing attention over the past few years. Gaier & Lee (61) reviewed studies in this area and concluded that there is considerable predictive advantage to be gained by giving adequate statistical recognition to patterns of test and inventory responses. Snodgrass (62) studied the reliability of test profiles and concluded that much of the unevenness of profiles could be attributed to test unreliability.

McQuary (63) factor analyzed the intercorrelations of 23 variables, including college grade points and several nonintellectual characteristics such as size of home community, number of siblings, health, etc., based on a sample of college freshman men. Two of the seven factors extracted seemed to be related to college achievement, one yielding positive loadings for such variables as size of home community and high school extracurricular participation. Although this factor did appear to be related to grades earned in college, the loading of .21 seems to the reviewer to be too low to identify the factor. Schultz & Green (64) developed an attitude-interest questionnaire intended to measure nonintellectual factors associated with academic achievement in college, which upon cross-validation yielded low positive correlation coefficients suggestive of a small, though stable, relationship.

Effects of guidance.—Serene (65) reported an attempt to motivate senior high school under-achievers to work closer to their levels of ability. Correlations between ability and achievement for two control groups remained about the same, while a group that was counseled throughout the year (given information on how to study, and other assistance) showed a change in ability-achievement correlation of from .56 to .76.

Johnson (66) reported vocational counseling to contribute significantly to knowledge of clients about themselves. The counseling appeared to enhance both accuracy and certainty of self-knowledge, the greatest gain being for intelligence and the least for certain personality characteristics.

MEASUREMENT

A number of significant discussions and reports relative to problems of behavior measurement and its impact on education have appeared. Although pertaining to educational psychology many of these are omitted here because they seemed to fall in the province of other chapters ("Individual Differences," "Theory and Technique of Assessment," and "Statistical Theory") of this volume.

General treatments.—An issue of the *Review of Educational Research* (67) was devoted to testing and the use of test results, covering such areas of test development and application as: general mental ability, special aptitude,

nonprojective tests of personality and interest, projective tests of personality, educational achievement in schools and colleges, and educational achievement outside the school.

At least seven new textbooks, or revisions of previously published textbooks of importance, were published during the period of the review. Greene, Jorgensen & Gerberich (68, 69) completely revised and brought up to date their books dealing with measurement and evaluation in the elementary and secondary schools. The first 385 pages (14 chapters) of the two books are identical, dealing with common problems of measurement. In the elementary volume the last 8 chapters deal with testing in the elementary school, and similarly in the secondary volume 15 additional chapters relate to applications of measurement to high school subjects and to general educational achievement. The coverage in both books is relatively complete, and technical as well as practical matters seem to be satisfactorily covered. Traxler *et al.* (70) brought out a handbook on the use of test results in schools. The materials relate primarily to commercially available standardized tests. Torgerson & Adams (71) attempted to describe measurement and evaluation for the teacher in the elementary school, leaning in emphasis toward matters of educational evaluation. On the technical side, the materials presented are not outstanding.

An important revision of a widely used text, Ross's *Measurement in Today's Schools*, was prepared by Stanley (72). The revision has been competently handled and has brought the book above the level of merely a popular text, making it one that is also generally sound. Innumerable changes also were made to bring the contents up to date and to increase their readability. Anastasi (73) provided a notable addition to a growing list of texts in mental measurement. Although the title, *Psychological Testing*, implies that the book is intended primarily for psychology students, it carries a great deal of important information for counselors, psychometrists, personnel workers, teachers, principals, and others. From the educator's standpoint the most notable omission, one common to most books on psychological testing, is a description of how to prepare test items and carry through test construction. Otherwise the treatment is complete, well-documented, and rigorous without being overly difficult. Jordan (74) also published a textbook, *Measurement in Education*, which probably will prove to be popular. In order of their accuracy, technical quality, comprehensiveness, and usefulness for educational psychology students, the reviewer ranks the books just named in the following order: Greene, Jorgensen & Gerberich, Anastasi, Ross-Stanley, Jordan, Traxler *et al.*, and Torgerson-Adams.

A joint committee of three professional organizations concerned with testing released an important report (75) dealing with technical recommendations for psychological tests and diagnostic instruments. In one sense the report constitutes a short review course in mental measurement for test users such as counselors, school psychologists, directors of research, and administrators, suggesting standards to be considered in the choice of tests

for use. The report also is intended for test authors and publishers, in the interest of the provision of more satisfactory test manuals that give adequate information for wise test choice and interpretation. Attention is drawn to recent developments in thinking about tests and test analyses through recommendations falling under six headings: dissemination of information, interpretation, validity, reliability, administration and scoring, and scales and norms. The recommendations are supplemented with commentaries which provide illustrative materials or statements intended to clarify.

Sundberg (76) studied references listed in bibliographies accompanying reviews of tests in *Buros' Mental Measurements Yearbooks* and tabulated references to tests according to period during which publication appeared. Through 1936, the Stanford-Binet, Rorschach, Seashore, Bernreuter, and Strong instruments were most frequently reported in the literature. In 1948-1951, the Rorschach, Wechsler-Bellevue, Minnesota Multiphasic Personality Inventory, Thematic Apperception Test, and Kuder Preference Record were most frequently reported. The trend appeared to be from aptitude measurement toward clinically oriented tests and inventories.

Appearance of *The Fourth Mental Measurements Yearbook* (77) is significant to note. This highly regarded and much used reference work covers the period 1948 to 1951 and attempts to list all commercially available tests published in English speaking countries during that period. As has been the practice, an effort was made to present the tests objectively with as complete information as possible, including references to the literature. Reviewers had been admonished to prepare reviews that were frankly critical and which would stimulate progress toward higher professional standards in test construction. Unfortunately no reviews were carried for a number of the tests listed.

Bean (78) published a volume on the *Construction of Educational and Personnel Tests*. The book contains little that is not mentioned in any good educational measurements textbook. It is poorly documented, fewer than 30 names or sources being cited in an index, notably missing being such names as Cureton, Flanagan, Richardson, Gulliksen, Cronbach, Davis, Tyler, Lindquist, Guilford, and others. *Educational Measurement*, the comprehensive volume published by the American Council on Education several years ago, is not mentioned.

What do tests measure?—Jones (79) factor analyzed the inter-item correlations of Stanford-Binet records for a sample of 13-year-old boys and girls, rotating to oblique simple structure. In general the results confirmed earlier analyses by the same author in which a smaller number of centroid factors were extracted and orthogonal rotation accomplished. Second-order factors, presented with due caution, also were computed; these appeared to relate to (a) "ability to profit from scholastic experience," (b) "ability to synthesize perceptions so as to impose a good Gestalt," and (c) "facility for dealing with relations."

Kolstoe (80) sought to determine whether there are significant ability

differences between children of the same mental age but varying chronological age (bright and dull). An effort was made to circumvent difficulties of earlier studies by matching low and high IQ groups on estimated true mental age scores (obtained by regressing their obtained MA scores) and also by avoiding use of the same test for the criterion that was used for matching. The Stanford-Binet was employed for matching, and the Wechsler Intelligence Scale for Children subtests, Benton Test of Visual Retention, and Chicago Tests of Primary Mental Abilities as criterion measures. Although there were some differences between the groups, the results seemed to support the generality of the MA concept; there seemed to be little evidence to support the hypothesis, frequently stated, that brighter subjects are superior in vocabulary, reasoning, and general memory, while duller subjects exceed in performance and manual manipulation.

Tyler (81) took issue with Eels *et al.* in their interpretation that a positive correlation between socioeconomic status and measures of general intelligence is obtained because commonly used test materials are biased in favor of high status groups. Consideration of such factors as test unreliability, inequality of IQ units for different tests, and varying difficulty levels of the tests employed, suggests the unsatisfactoriness of the evidence presented and points to the need for an experimental approach to answer the question.

Dolch & Leeds (82) compared existing vocabulary tests from the standpoint of adequacy with which they measured children's knowledge of word meanings, concluding that most tests do not satisfactorily measure vocabulary, that they ignore all but the most common meaning of a word (and often get at very little of that most common meaning) and that they measure a very indefinite amount of information.

Jones (83) analyzed reasons for students' responses to achievement test items after having them write justifications for their choice of correct and incorrect alternatives to multiple-choice items. Errors appeared to be attributable chiefly to failure to see the problem clearly, to application of the wrong principle to the solution, and to lack of factual information.

Coaching and measurement.—Wiseman & Wrigley (84) studied the comparative effects of coaching and practice on verbal intelligence tests, finding a mean IQ gain for a control group of 4.5, for a coached group of 6.5, and for a practice group of 11 points (the first five practice tests producing a gain of 9.5 IQ points). Practice was most effective with those of high initial IQ, and coaching was most effective with children of low IQ. Gagné (85) discussed a related problem, noting that the same instrument (either paper and pencil tests or performance tests) is sometimes used for measurement and also for improvement of performance (practice). It is pointed out that reliability and validity are the important characteristics of a device when it is used for measurement, whereas amount of transfer produced (to a operational task) is the important characteristic of a device used for improving performance.

Personality assessment.—Vernon (86) presented what is probably one of

the better of recent descriptive treatments of personality measurement of the normal individual. The book is somewhat like Symonds' *Diagnosing Personality and Conduct* except that it is more concise and, of course, includes much recent material. The major emphasis is upon "trait" estimation. Clinical tests are dealt with only briefly. French (87) attempted to bring together comparable factor analyses in the field of personality, paralleling his earlier publication dealing with aptitude and achievement tests. Some 49 personality factors are listed which have been identified in at least two analyses with sufficient certainty to be named.

Cattell & Gruen (88) prepared a personality inventory for children from 11 to 14 years of age. The questionnaire is based upon a factor analysis of intercorrelations of certain discriminating items from a larger pool.

Berg & Collier (89) studied the extreme responses ("like much"; "dislike much") of subjects to a *Perceptual Reaction Test*. Greater frequency of extreme responses was found for white females as compared to white males, for Negro males as compared to white males, and for high anxiety males as compared to low anxiety males. The data suggest that extreme response scores may reflect personality and group characteristics.

An improved formula for scoring Guess Who ratings was proposed by Keislar (90). The new formula weights "number of mentions" in inverse proportion to how widely an individual is known and yields a score on a trait even though an individual is not mentioned on that trait provided he is mentioned on other traits. The new method of scoring was compared with previously recommended formulae; it was found to be the only method yielding normal trait distributions. It also yielded higher validity coefficients for such traits as "likes school work" and "puts study first" with school grades as the criterion.

Teachers' marks.—Carter (91) investigated the relationship between certain variables and course marks assigned by teachers to students of beginning algebra. Personality assessments, interests, and age appeared to have little bearing on marks. Socioeconomic status and marks were inversely related. Girls seemed to have some advantage over boys, this being true regardless of whether the teacher was male or female.

Nedelsky (92) suggested a technique for determining the minimum passing score (and the minimum score for grades of *A*, *B*, *C*, and *D*) on a multiple-choice test, based on the instructor's judgment of what constitutes adequate achievement. Basically, this method requires an instructor to judge and mark all responses to a multiple-choice item that he believes the lowest *D* student (barely passing) should be able to reject as incorrect, and then to note the reciprocal of the number of remaining responses (e.g., if two choices are marked, for a five choice item, the reciprocal $\frac{1}{3}$ is noted). The probable mean score of borderline (*F-D*) students is equal to the sum of the reciprocals of the numbers of responses other than *F* responses (providing a "guess score," or sort of an "absence of ignorance" standard). Adjustments may be made to fail any percentage of the *F-D* students judged appropriate, the

passing score being determined by the formula $\bar{M}_{FD} + k\sigma_{FD}$, where \bar{M}_{FD} is the average of the M_{FD} of various instructors. An approximation formula which will pass 16 per cent of the borderline ($F-D$) cases is: Minimum Passing Score = $\bar{M}_{FD} + .45\sqrt{N}$. Smith (93) also provided a description of how scores relating to different components of a course may be combined and grades assigned.

RESEARCH METHODS

Research emphases.—Coladarci (94) questioned the meaningfulness of much research being conducted in educational psychology and education in general and pointed out that research conclusions frequently suggest a one-to-one relationship between a specific educational procedure and an educational situation, whereas such a relationship is unlikely in light of the relative instability of behavior data, systematic differences in behavior theory, and variations in educational purposes. Also, it is maintained, many researches have no strategic relevance to the educative process and many researchers are insensitive to the valuational dimension in research. Freeman (95) observed that research frequently has been casual and haphazard. Its improvement may be effected by attention to planning, emphasis on important rather than trivial issues, and penetration and analysis to get at explanatory principles beneath surface results.

Methodology of research.—A number of general presentations of research methodology in educational psychology and closely related fields have appeared. Barr, Davis & Johnson (96) published a new textbook dealing with *Educational Research and Appraisal*. A chapter on "The Sampling Survey" appears to be outstanding; it is one of the few satisfactory treatments of research procedures provided by the book. Good & Scates (97), in *Methods of Research*, observe an increasing interdependence of problems and procedures of educational, psychological, and sociological investigations and, accordingly, attempt to cover common patterns of research methodology in the several fields. The material provides interesting reading, but in an apparent attempt to avoid the "cook book" style of presentation, the authors have leaned in the direction of presenting a "philosophy of research" and in doing so have become somewhat wordy and expansive. The chapters dealing with "descriptive methods" appear most adequate. Statistical techniques are omitted and relatively little space is given to experimental methods.

Festinger, Katz *et al.* (98) describe, within the basic logic of scientific methodology, techniques applicable to subject matter in the social and psychological, or behavioral sciences. A number of chapters reflect the technical competence of their authors, and the book as a whole is one of the better ones. Education students in the reviewer's classes had some difficulty, however, in adapting to the terminology and frame of reference.

Ackoff (99) published a research textbook with a number of noteworthy features, though it is somewhat limited from the standpoint of research techniques available to educational psychology. The discussion of the formu-

lation of hypotheses, of the nature and importance of operational definitions, of Type I and Type II errors and their reduction, and of sampling designs provide good research foundations. A new book on statistical inference by Walker & Lev (100) should be particularly useful to graduate students and researchers in educational psychology. The presentation is somewhat intermediate in content and difficulty between the elementary statistics text and advanced treatments dealing with experimental design. Lindquist (101) has expanded his earlier pioneer description of applications of the analysis of variance and covariance to educational problems in a well-developed treatment of experimental design. The work seems to be rigorous and sound, although it will be too difficult for many graduate students in education who lack statistical background. However, together with Walker & Lev, this volume points the way toward tools the educational researcher must become acquainted with and must use if useful answers are to be found to many of our educational questions, and if educational practice is to have a truly scientific basis.

TEACHER BEHAVIOR

Criteria of teacher effectiveness.—A second report of a committee of the American Educational Research Association (102) suggested a number of considerations relative to the investigation of teacher effectiveness. Common research problems or difficulties were noted and certain factors to be taken into account in evaluating criteria of teacher effectiveness were reviewed. Ryans (103) proposed that teaching is effective to the extent the teacher does things, or behaves, in ways that are favorable to the development of skills, understandings, work habits, desirable attitudes, and adequate personal adjustment on the part of the pupils or students. But it is difficult to isolate criteria in accordance with this definition, because specific skills, understandings, and attitudes often vary with the culture and even with the community and school system, and since our society (and researchers, as well) find it difficult to separate the process of teaching from the content taught, criteria of effective teaching are almost hopelessly relative and dependent upon the value system accepted in a particular place at a particular time. For this reason, it may be advisable for educational practitioners and researchers to direct their attention to criteria in specified areas of teaching behavior rather than to criteria of teacher effectiveness per se.

Investigations of teacher characteristics.—One effort directed at the identification of characteristics of different kinds of teachers has been conducted under the auspices of the American Council on Education and The Grant Foundation (103). The objectives of this investigation have been (a) the identification of basic patterns of teacher behavior in the classroom and (b) the development of psychometric instruments for the prediction of identifiable patterns of teacher behavior. Factor analyses of trained observers' assessments of teachers' classroom behavior led to a designation of three major patterns, one having to do with "friendly versus aloof," another with "sys-

tematic versus poorly organized," and a third with "stimulating versus dull" teacher characteristics. Teacher attitude and teacher belief variables (attitude toward pupils, attitude toward school personnel, acceptance of "traditional" versus "modern" educational philosophy) also were studied. A number of types of predictor materials were developed and analyzed against the criteria having to do with classroom behaviors and attitudes. Various group differences also have been studied.

Bendig (104) intercorrelated rating scale data on 10 introductory psychology instructors and performed an inverted factor analysis. Three factors were extracted. The author does not attempt to assign rubrics to the factors, but the reviewer hazards a description of the first factor as "stimulating, interesting," the second factor as "organized, systematic," and the third factor as "appreciative, friendly." The factor descriptions were validated against rankings made on the three factors by competent judges, resulting in a mean validity of .49. The rough correspondence of these factors, obtained in a study of college teachers, with those found among elementary and secondary teachers by the Teacher Characteristics Study (103) is perhaps noteworthy.

Blanchard (105) studied a random sample of some 800 educators whose biographies appeared in *Who's Who in American Education* and analyzed the data from a number of viewpoints. The birthplace of members of this sample was most frequently in the North Central and Midwestern states. The bachelor's degree most frequently had been obtained from the University of California or the University of Minnesota; the Master's and Doctor's degrees most frequently from Columbia University. It should be noted that the volume *Who's Who in American Education* provides a biased sample of American educators and is neither representative of American educators at large, nor of individuals who have made outstanding contributions.

Trabue (106) reported on the administration to a group of 820 college executives of a check list having to do with the desirability of certain traits in lower-division college teachers. Some of the traits most frequently marked as being of great value were "inspires students to think for themselves and to express their own ideas sincerely," "is emotionally stable and mature," "is friendly, democratic, tolerant, and helpful in his relations with students," and "organizes materials and prepares carefully for each meeting of the class." Some of the characteristics marked as having little value were "engages actively in political work," "is less than 35 years of age," "was unsuccessful in a nonacademic job," "has been successful as elementary and secondary school teacher," and "holds a graduate degree from a 'noted' university."

Ryans (107) presented the results of factor analyses of elementary teacher and secondary teacher responses to an educational viewpoints inquiry (intended to determine aspects of educational philosophy). The results suggest that teachers' educational viewpoints are not highly systematized although one or two clusters are suggested. Oliver (108) asked elementary school teachers to indicate, first, their own educational beliefs, and, second, the ex-

tent to which these beliefs were carried out in the classroom. It was concluded that the pattern of modern educational philosophy was accepted by the group, but that this philosophy was not implemented in the classroom.

Callis (109) administered the Minnesota Teacher Attitude Inventory to teachers in grades 4 through 10. The results were found to be correlated significantly with pupils' ratings and observers' ratings of the teachers' interpersonal relations, but not with principals' ratings. This finding suggests that the principals' ratings may have been contaminated by characteristics of the teachers other than those the principals were attempting to judge. Fink (110) studied principals' ratings of teachers, the teachers having been administered the California Ethnocentrism Scale, assumed to be an indicator of authoritarian personality. A moderate but statistically significant correlation between principals' ratings and ethnocentrism suggested that the principals tended to give favorable ratings to teachers characterized by rigid authoritarian, and conforming personalities.

Grim, Hoyt & Mayo (111) reported further studies of a Teacher Characteristics List and Student Reaction Inventory developed at the University of Minnesota. Among other findings it was noted that scores on the Student Reaction Inventory did not appear to be related to a principal's rating of a teacher, a teacher's self-rating, age of teacher, teaching grade level, or subject matter field. The instrument appeared to yield relatively unique information about teacher behavior.

Knoell (112) studied the relationship between word fluency and teaching success. Ideational fluency (fluency for adjectives and for "things round") correlated highest with ratings of teaching success, and suggested that personality characteristics associated with fluency may be factors in the teacher's performance. Studies conducted under the reviewer's direction in connection with the Teacher Characteristics Study also have indicated a positive relationship between fluency and scores on patterns of teacher behavior, particularly "friendly-kindly" behavior.

The prediction of student-teaching success from personality inventory results was studied by Tyler (60). The study is outstandingly thorough from a technical standpoint. In general, the materials employed showed little evidence of being useful for predicting student teaching success as measured by supervisors' ratings. It is unfortunate that so rigorously conducted a study should not have had the advantage of more satisfactory criterion data.

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PROBLEM SOLVING AND THINKING¹

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The increasing interest of psychologists in problem solving and such closely related areas as concept formation, decision making, and creative thinking is shown by the large number of papers published within the last five years. It is further demonstrated by the fact that of about 125 relevant doctoral dissertations (154) completed in the United States during 1949 to 1953, about 35 appeared within the most recent year. Of about 60 papers on this subject presented at meetings of the American Psychological Association during the same period, 20 were given in 1953. Last April a conference on Human Problem Solving, sponsored by the National Science Foundation with New York University as host, was attended by more than 40 psychologists.

Limitations of space and a personal preference for dealing more adequately with fewer areas have combined to make the present review selective, not only with respect to articles within areas but also with respect to areas covered. Among the areas omitted are those involving problem solving and thinking in children [e.g., Piaget & Inhelder (118, 119); Russell (135)]; in neurotic and psychotic individuals [e.g., Epstein (33); Welch & Diethelm (160)]; in individuals with brain injury [e.g., Battersby *et al.* (6); Reitman & Robertson (127)]; and in animals [e.g., Harlow and associates (62, 109); Hymovitch (76); Maier & Longhurst (94)]. The factor analysis of reasoning abilities, as represented by the work of Adkins and her associates (3, 103), Guilford and his associates (45, 49 to 53), and Corter (26), has been reviewed in the chapters on Individual Differences in this and preceding volumes and, for that reason, will not be included here. The authors perhaps regret most the necessity of omitting the area of creative thinking [Guilford and associates (48, 54, 55, 163); Green (44); Hadamard (56); Lehman (82); Roe (128 to 131); Thurstone (149, 150)]. Following a brief reference to recent books and a short summary of research on concept formation, the present chapter will give primary attention to problem solving by normal adult humans, individually and in groups.

The Psychology of Thinking by Vinacke (158) is essentially an under-

¹ This review covers the five-year period beginning about July, 1949, the termination date for Johnson's earlier review (77), and ending about June, 1954. No reference has been made to the unpublished doctoral dissertations (with one exception) or to the papers presented at meetings of the American Psychological Association. This review was prepared as part of an experimental program of research on thinking being carried out under Projects NR 150-104 and NR 153-149, supported by Contracts Nonr 225-02 and N6onr 25125 between Stanford University and the Office of Naval Research. Work on both contracts is under the general direction of the first author. Permission is granted to the United States Government for reproduction, translation publication, use, and disposal of this article in whole or in part.

graduate text and covers a broad range of topics from consciousness through problem solving to creative thinking. Roughly one-third of Humphrey's *Thinking* (75) is devoted to a review and evaluation of the work of the Würzburg Group. Attention is also given to association theory, the work of Selz, and Gestalt theory as well as to motor reaction, language, and generalization. The final chapter attempts to summarize, in a series of 16 brief statements, the results of the past 50 years of work on thinking. Under the title, *Organization and Pathology of Thought*, Rapaport (125) has translated and extensively annotated some 27 papers, most of which were originally published in German between 1905 and 1940. Included are papers by Ach, Buehler, Claparede, Lewin, Piaget, Freud, Bleuler, Schilder, and also a final chapter by Rapaport entitled "Toward a Theory of Thinking." All three books include extensive bibliographies. Comparing them, one is impressed by how different three treatments of the same general topic can be. They appear more similar if one includes in the comparison another recent book, *Innovation*, by the anthropologist, Barnett (5).

CONCEPT FORMATION

Johnson's chapter (77) and Vinacke's critical review (157), which was brought up to date for inclusion in his book on thinking (158), cover most of the research on concept formation through 1951. In briefer treatments, both Hebb (64) and Wright (165) have emphasized the importance of the intentional aspects of concepts; Underwood (155) has offered several research predictions based on the "one essential assumption" of contiguity, and Harlow has affirmed (60, p. 469) that "Breadth rather than intensity of training appears to be the key to efficient concept formation whether we deal with subhuman or with human subjects."

Heidbreder (68) has replied to the Dattman & Israel (31) criticism that her materials do not supply "equivalent perceptual instances" uniformly over the three categories for which she had demonstrated (65, 66, 67) the following regular order of attainment: concrete objects, forms, and numbers. Heidbreder asserts that she has been misinterpreted by her critics as is evidenced not only by their substitution of the terms "conceptual capacities" and "perceptual equivalences" for her terms "conceptual processes" and "situational support," respectively, but also by their implication that she had assumed that her drawings were equal in perceptual effectiveness. Furthermore, she contends that their experiments do not test her hypothesis since they failed to provide "restricted situational support" and that their "single broad principle" does not explain the results of her experiments.

Of interest here is the fact that an experiment by Grant, Jones & Talantis (43), which had indicated greater ease of attainment for numbers than for either form or color concepts of the Wisconsin Card Sorting test, when repeated by Grant (42) with better control of materials confirmed Heidbreder's order. Also in agreement as regards obtained order is Baum's study (7) for which a set of the Heidbreder cards was used. However, Baum's

order was predictable on the basis of the number of intrusion errors per stimulus, and the results were interpreted (p. 94) "as supporting the interconcept intralist interference implications of a stimulus-discriminability hypothesis." Which of the two hypotheses, discriminability or order of dominance, has the greater utility will, of course, depend on which is eventually shown to have the greater generality.

Baum's study is indicative of a major trend of research in this area to extend learning theory derived from the study of behavior at lower levels to the interpretation of concept formation. Buss (16), who claims to have presented the first account of concept formation in terms of modern learning theory, succeeded in deriving a gradient of stimulus generalization in terms of the errors made on a concept formation criterion series by five groups, each of which was pretrained with a different proportion of positive and negative instances. It was demonstrated that the more stimuli of one kind presented in the learning series, the greater the accuracy in categorizing these stimuli. More recently Oseas & Underwood (116), by analyzing errors made during learning, were able to establish a gradient of stimulus generalization along a size dimension; concepts based on large and on small forms were more rapidly attained than those based on medium-sized forms. A second major finding of this study was that distributed practice in concept formation proved, as in the case of rote learning, to be superior to massed practice.

The fifth (126) in Reed's sequence of studies on the learning of concepts deals with the relative economy of simultaneous and serial presentation for varied lengths of series. The former method proved significantly more accurate, but required significantly more work per concept than the latter for a 24-card series. However, as the series were lengthened differences both in accuracy and in work required approached zero. A study by Davidson (32) of the quantitative effects of three variables (level of abstraction of symbols, shift, and manipulation) on efficiency of concept attainment gave positive results only for manipulation.

Hovland (72) has made an interesting "communication analysis" of a concept model with stimuli specified in terms of a number of "dimensions" and "values." Analysis of this model permitted exact determination of the number of each type of instance, positive and negative, required to transmit correctly the concept characteristics under various combinations of specified values along two or more stimulus dimensions and led to the conclusion that the relative effectiveness of the two types, in terms of the number of instances required, was dependent upon the conditions. Using the data derived from this analysis, Hovland & Weiss (74) equated the amount of information conveyed by positive and negative instances and thus were able to determine the difference between the two types in difficulty of assimilation. All-positive instances were shown to be consistently superior to all-negative instances, which were in turn superior to mixed instances, in the transmission of information. In the case of all-negative instances, simultaneous presentation produced greater accuracy than serial.

* Should be mixed. See Errata before Contents
v. 7, 1956.

* Should be all-negative (?) See Errata before "
v. 7, 1956, p. 456, line 43.

We can here only call attention to the interesting study by Marks & Ramond (101) of concept evocation under "textbook" and "real life" situations, to Hanfmann's recent exposition (57) of the Hanfmann-Kasanin Concept Formation Test, to Hovland's new set of flower designs (73) which permit categorization on the basis of four values along each of four dimensions, and to the projective technique involving drawing of "the most unpleasant concept," contributed by Harrower (63).

SET, "RIGIDITY," AND FUNCTIONAL FIXEDNESS

The meanings, methods of measurement, and interpretations of the operation of set are diverse indeed. Woodworth & Schlosberg (164), though well aware of the disadvantages of set under certain problem-solving conditions, deplore the implications of "fixity and inflexibility" imputed to the word "set." However, the major part of recent research concerns itself with this negative aspect of set. The impetus to this trend was no doubt afforded, albeit unwillingly, by Luchins, who has reported extensive studies of Einstellung phenomena in problem solving, the first of which (85) appeared in 1942.

Einstellung test.—Since nearly half of the studies to be reviewed here employ some form of the water-jar test, a brief description of the problem series as outlined by Luchins (85, 88) is appropriate. For each problem the volumes of three jars, a , b , and c , are specified; the task is to indicate the manipulation of jars necessary to obtain a specified quantity of water. Subjects are told that they need not use all three jars. The first problem is illustrative; the next five, "Einstellung" problems which induce set, are solvable only by the set method ($b-a-2c$); the next two, "criticals" which test for the establishment of set, are solvable either by the set method or by a more direct method ($a-c$ or $a+c$); then follows the "extinction" problem, which is solvable only by the direct method and which tests the strength of the set [or the "ability to overcome set," as Guetzkow (47) has put it]; finally there may be two additional "criticals," which are compared with the first two criticals if one wishes a measure of the degree of recovery from set. A survey of the literature reveals that the actual number of Einstellung, critical, and extinction problems used differs from study to study, but the most common variation is to omit the extinction problem and use the critical problems as a measure of "rigidity" or inflexibility.

It is this variation to which Luchins has raised strong objections. Luchins' criticisms were first (86) directed at Rokeach's study of rigidity and ethnocentrism, but they have since (87) been readdressed to all whom "the shoe fits." Rokeach (132) defined rigidity as "the inability to change one's set when the objective conditions demand it . . ." and then used number of set solutions for critical problems as his measure of rigidity. Luchins argues that this measure does not satisfy Rokeach's definition of rigidity since a set solution is not incorrect, or even necessarily less efficient, and since there are many reasons (85, 87) other than rigidity for continued use of the set method.

Levitt & Zelen (83) have studied the problem of the "demand aspect" of the criticals and have presented evidence that the direct method is not more efficient than the set method in terms of time, when the test is administered under any one of their three experimental conditions. Similarly, Tresselt & Leeds (152) found that use of the set method speeds up solutions.

Luchins asserts, "Einstellung refers to set and not to the ability to change one's set or to recover from a set" (86, p. 456). That the critical and extinction problems do not measure the same trait is strongly suggested by the very illuminating experiments of Guetzkow (47) whose work was reviewed by Johnson (77) before its publication. Pertinent here is Guetzkow's finding of a significant sex difference in the "ability to overcome set" but no difference in "susceptibility to set" on the water-jar test. Evidence that the former is related whereas the latter is unrelated to other variables has been offered by Adamson & Taylor (2) for "functional fixedness," by Luchins (88) for concreteness of thinking, and by McNemar (105) for logical reasoning. However, Sweeney (144) failed to demonstrate a sex difference in ability to overcome the set established by a series of trick problems, and Guetzkow (47) found that those who overcame set on the Luchins series did not give more solutions to the Maier two-string problem. The separation of susceptibility to set from ability to overcome set may be specific to the type of problem.

Generality of set or rigidity.—This fundamental question has attracted much attention. Fisher (36) in an overview of research trends makes a plea for postponement of the determination of innumerable correlates until after the range and complexity of rigidity phenomena have been systematically investigated. The failure of Kleemeier & Dudek (80) to isolate a flexibility factor was discussed by Johnson (77), but since then the results of three new factor analyses have been reported. Cattell & Tiner (21) concluded from their analysis of 17 tests that there are possibly three rigidity factors in addition to low-fluency and low-intelligence; only one, "process" rigidity, or the classical perseveration factor, is adequately defined. Hypotheses as to the nature of "structural" rigidity are given by Cattell & Winder (22) following a survey of rigidity in clinical and experimental areas.

Making a distinction between cognitive-process tests demanding reorganization of experimentally induced patterns and those demanding reorganization of culturally induced patterns, Oliver & Ferguson (115) isolated a "habit interference" rigidity factor on which several tests of the former type had appreciable loadings. But the Luchins series gave a zero loading on this factor. An analysis by Scheier & Ferguson (137), including motor speed and cognitive process tests, yielded no justification for generalizing from motor to nonmotor behavior; the results gave fair confirmation of the Oliver & Ferguson rigidity factor.

The remaining evidence pertinent to the generality question is based on magnitudes of correlations found between various rigidity measures and on degree of differentiation of rigid and nonrigid groups by thought-to-be-related variables. Using number of set solutions (on critical plus extinction

problems) as the rigidity score, Cowen, Wiener & Hess (30) concluded, on the basis of a phi coefficient of .46 between the water-jar test and an identically arranged alphabet-maze test, that "under specific and constant field conditions . . . a tendency toward a generalized mode of problem solution has been demonstrated and the likelihood that this tendency is 'personality-related' strengthened" (p. 102). Later, with criticals only as the criterion, Cowen & Thompson (29) found that rigid and nonrigid groups differed significantly on each of several Rorschach "factors" but did not differ on judges' pooled rigidity ratings, based on the same Rorschach records. The results for "factors" rest on raw-score comparisons which (despite the authors' curt dismissal of the fact) are spurious because of the significant difference found for total number of Rorschach responses. More cautious is Fisher, who writes "that the concept of general rigidity as a real entity is to a considerable degree fictitious and that one should probably not anticipate more than very moderate success in attempting to measure such a variable" (37, p. 41) even though the correlation of total scores on his battery of 10 rigidity tests with Rorschach rigidity scores shows the former to be a "moderately good measure" of rigidity trends.

Goodstein is emphatic in his conclusions on the generality of rigidity: "(a) it is difficult to maintain a concept of rigidity, even intellectual rigidity, as a unitary psychological trait, and (b) intellectual rigidity, as it has been conceived, is not a necessary concomitant of extreme or stable social attitudes" (40, p. 352). All of which is understandable in view of his experimental results. The average intercorrelation of three tests of rigidity, the water-jar test (criticals), the Shipley-Hartford Scale, and an anagram set test, is .09; the average for the intercorrelations of the rigidity tests with four Thurstone attitude scales, law, censorship, patriotism, and the Bible, is .02; and those rigid on all three rigidity tests "could not be differentiated on any variable" from those low on all three tests.

Solomon finds rigid and nonrigid groups (criticals) differ significantly as regards: (a) scores on a test of "ability to utilize the elements of the scientific method" (142), and (b) comprehensiveness of "cognitive patterns" (143). Pitcher & Stacey (120) patterned a word-similarities test after the water-jar test. Subjects classified into seven rigidity groups on the basis of this test differed on only one of nine personality traits of the Guilford-Zimmerman Temperament Survey. That the more rigid were more submissive prompted the authors to indicate that ego-involvement may be the fundamental determinant of rigidity in problem solving situations. Constructing her own scale of rigidity, Wesley (161) compared the performances of rigid, anxious, and normal groups on a concept formation test. Comparisons reaching significance are as follows: the rigid were slower than the normal in forming new conceptual sets; the rigid made more perseverative responses than the normal, who made more than the anxious. Open to criticism are Wesley's method of selecting extreme groups and, in view of large differences in variance, her score corrections for original learning. Moldowsky (112) failed to find a rela-

tionship between judges' ratings of behavior in a Moreno Spontaneity Test situation and scores on Wesley's rigidity scale. He feels that this casts doubt on the validity of Wesley's R-scale and suggests that her item selection criterion, "clinicians' notions of what rigid people would say about themselves," may be at fault. But the rigidity in Moldowsky's Spontaneity Test is based on judges' ratings of behavior!

The announcement by Rokeach (132) of the relationship of rigidity and of concretizedness to ethnocentrism has provoked other efforts to reproduce his results. Brown (14) demonstrated that the association of authoritarianism (California F scale) and rigidity on critical problems is dependent upon the establishment of an ego-involving atmosphere and also that both authoritarianism and rigidity are associated with moderate *n* Achievement, as measured by a special scoring of Thematic Apperception Test stories. Brown suggests that this "associated" rigidity is a kind of defense against personal failure on the part of the ethnocentric. In contrast to these results are those of Levitt & Zelen (83), who found that the relationship between rigidity (also on criticals) and ethnocentrism is dependent upon a relaxed, or "free" (unsped, unrewarded), atmosphere and that the relationship does not exist under speed-incentive conditions. It is too bad that their speed condition was confounded with reward, which may have counteracted the increase in set solution reported by Luchins (85) for speed conditions. In neither study was the E scale, employed by Rokeach, used (the E and F scales correlate about .70). One wonders in view of Luchins' criticisms (86, 87) why both these investigators failed to include an extinction problem. It would be informative to have results on both criteria under unconfounded "atmosphere" conditions. The third experiment aimed at checking Rokeach's results is Luchins' (88) attempt to get at the relation of rigidity and concretizedness of thinking. His analysis leaves much to be desired, nor are enough data given to permit reanalysis. The observed trends indicate that, when extinction is the criterion of rigidity, the rigid give a larger percentage of abstract responses on the Similarities Test of the Wechsler-Bellevue but that, with criticals as the criterion, rigid and nonrigid do not differ in this respect.

Brown's succinct statement that rigidity is "a term which should always be written with an operational subscript," (14, p. 469) seems to us an effective summary of research results to date on the generality of rigidity.

Relation of set to other variables.—The effect of various field conditions on set or rigidity has been the object of several experiments. In Luchins' (87) opinion "the chief finding" of experimentation with the water-jar series is the fact that set performance is influenced by field conditions. During the period covered by this review, Luchins & Luchins (89) have reported on the effects of: (a) having the subjects keep track of fluid remaining after each manipulation, (b) adding a fourth jar to the series, and (c) "concretizing" by allowing actual jar manipulation. None of these conditions resulted in a significant decrease in set solutions (criticals and extinction). In another study (91) an Einstellung maze series requiring mirror tracing was used. The failure

of the relatively greater effort necessary for tracing the set path to produce increased use of the direct path is explained in terms of field-theoretical concepts and is said not to support either an unqualified "law of least effort" or Hull's reactive inhibition theory.

Tresselt & Leeds (152) have confirmed the observation that set solutions of criticals occur about equally under concrete and abstract conditions. These authors also (a) investigated (153) the relationship between number of Einstellung problems in the series and strength of set (criticals), and (b) tested the retention of set after one, two, and seven days. The hypothesis that reactive inhibition will be induced only after a short period of facilitation is confirmed by a straight-line curve showing discontinuity after six set problems. The hypothesis that different periods of delay between onset of the habit and testing of the habit should result in different amounts of inhibition is less clearly substantiated; however, there is some evidence that the effect of time on the memory trace of the set is greater in the early part of the curve.

Comparing four groups required to "perceive" each set problem for 10, 20, 30, or 60 seconds before writing down the solution, Rokeach (133) finds that both rigidity (criticals) and concretemindedness, as indicated by quantity of scratch paper used, decrease with increasing perception time up to 30 seconds. This seems not at all inconsistent with Luchins' (85) results indicating that there is an optimum working-time limit for decreasing set solutions.

Differential task instructions are shown by Maltzman & Morrisett (98) to be independent of amount of training in their tendency to increase set solutions of anagrams. That sets produced by different instructional situations have differential effects on performance is also demonstrated by Luchins & Luchins (90) for the Wechsler-Bellevue, by Mech (106) for simple arithmetic operations, and by Fattu & Mech (34) for gear-train apparatus "trouble shooting." However, negative results were obtained by Mech, Schaerer & Auble (107) for a routine coding task. Weaver & Madden (159), Guetzkow (47), and Levitt & Zelen (83) failed to find hints as effective as would have been expected from the earlier work of Maier (92) and of Luchins (85).

The necessity for controlling set conditions has been emphasized by Postman & Crutchfield (124) who studied the relationship of hunger intensity and food-response frequency under three conditions of stimulus ambiguity (words having high, low, or medium food-response probabilities) and five degrees of set (0, 1, 2, 3, and 5 forced food responses in pretest series). Regardless of hunger level, the effects of set per se were of a larger order of magnitude than the differential effects of hunger. Set was shown to have its maximum effect when the probability of critical response was neither very high nor very low.

In addition to the work on atmosphere and problem solving rigidity, already reviewed here (14, 83), several papers concerned with rigidity as a function of anxiety have been published. Beier (9) presents evidence that perceived threat, anxiety induced by unfavorable interpretation of Ror-

schach protocols, is followed by a "loss in flexibility of intellectual function and a disorientation of visual-motor coordinations" (p. 19), as measured by standard tests of vocabulary, abstract reasoning, concept formation, and mirror tracing. Differences in set solutions to water-jar criticals plus an extinction problem (and to the latter alone) given by control, mild-stress, and strong-stress groups lead Cowen (27) to conclude that increasing stress elicits increasingly rigid problem-solving behavior. Strong stress was induced by interpretation of Levy Movement protocols whereas mild stress was induced by interjection of a puzzle-failure experience between the examples and the set problems. The use of set solutions by the stress groups is interpreted by Cowen as "ego-redeeming" and as providing a "pseudo security." Reported elsewhere (28) are the results of a comparison of this same strong-stress group with a group in whom strong stress was reduced by praise. Rigid solutions were significantly less in the latter, which we note proved more rigid than either the control or mild-stress groups of Cowen (27).

Osler's (117) purpose in designing her experiment was to study the differential effects of failure stress related to the test situation and of fear induced by an unrelated disciplinary threat situation. After a tremendous amount of work with over 300 subjects, Osler admits that her fear group is confounded with failure. Five groups were tested: failure, failure-fear, success-fear, success-control, and control. In spite of the omission of a fear group an analysis of covariance of the first four groups permits her to conclude that "failure as an experimental variable depressed performance . . . The other experimental stress, fear, did not affect performance significantly" (p. 118). She conjectures that failure serves to make subjects more ego-involved regarding the test than does threat of discipline. This is an interesting hypothesis.

Using groups scoring above and below the median on the Taylor Anxiety Scale, Maltzman, Fox & Morrisett (99) conducted two experiments. In the first a set for incorrect responses to a water-jar series was established, in the second a set for correct responses to a series of anagrams was established. In the former the high anxiety group gave significantly more set solutions, but in the latter the high anxiety group made significantly fewer errors. The authors conclude that manifest anxiety increases the effective drive state, multiplying all habits present at the moment, and thus produces an increase in set solutions or a decrease in anagram errors depending on whether "the dominant habit structure is congruent" with the incorrect or the correct response. They claim that Cowen's results can be accounted for by this theory. This raises a fundamental question: Is experimentally induced anxiety the same as manifest anxiety measured by the Taylor Anxiety Scale?

Set and learning theory.—Attempts to explain the phenomena of mental set on the basis of learning theory have been made by several investigators. A so-called "extinction hypothesis of mental set" proposed by Kendler, Greenberg & Richman (79) is said to reconcile their experimental findings with the generalization that distributed is superior to massed practice.

Strength of set as measured by a Luchins critical problem proved greater in a group given massed practice with set problems than in a group allowed three-minute intervals between set problems. This is said to be understandable since the learning of a mental set reflects mainly the extinction portion of the learning process and since massed practice is superior to distributed in experimental extinction. The authors predict that in concept formation a previously successful response would be rejected more rapidly under massed practice.

In an effort to discover whether certain deductions from Hull's principle of habit strength would hold for the phenomena of mental set, Maltzman & Morrisett (96, 97) performed two experiments. In the first, the prediction that "there should be an inverse relationship between amount of practice with incompatible classes of anagrams and subsequent success with one of them" (96, p. 243) was confirmed. The reduction of errors from first to second half of the test series was significantly greater for a weak-habit than for a strong-habit group. The second study investigated the implications for mental set of Hull's corollary concerning the summation of habit structures. Two single classes and one compound class of anagrams were used, and each of seven groups was given a different treatment as regards the training-testing anagram class and order of class. Three of six specific predictions were substantiated, and failure of the others could be rationalized on the basis of obtained, but unforeseen, significant differences in the two single classes. Two studies already discussed (98, 99) were designed to demonstrate that nonassociative factors serve along with habit strength to direct thinking.

Schroder & Rotter (138) showed that varied training sequences resulted in differences in the readiness with which subjects learned to shift from one solution to another. Those whose training series involved the most shifting were least rigid, whereas those trained to sort on one aspect only were most rigid, on the test trials. The authors conclude that reward following nonreward not only strengthens or weakens a particular habit but also increases expectance that rewards follow nonrewards. Buss in three experiments studied rigidity, defined as "resistance to shifting from old to new discriminations" (17, p. 226), and found an inverse relationship between strength of response in a reversal series and number of nonreinforcements in the prereversal series. Buss (18, 19) also showed that reversal shift and absolute shift, when compared with nonreversal shift and relational shift, respectively, resulted in greater readiness to shift. These results are interpreted in terms of reinforcement inhibition.

Explicit recognition should be given here to the importance of the work on "learning sets" initiated by Harlow (58) for understanding the role of insight in problem solving. This work, however, has been discussed in the chapters on Learning in previous volumes of this Review.

Functional fixedness.—Three of Duncker's well-known experiments, which a decade ago yielded evidence that use of an object in one function in-

hibits solution of a problem requiring its use in a different function, were repeated by Adamson (1). With larger N 's and more clearly specified conditions, he obtained results which essentially confirmed Duncker's findings.

This negative effect of past experience was further demonstrated by Birch & Rabinowitz (11) using Maier's two-string problem. Two experimental groups, both pretrained in completing an electrical circuit, one with a relay and the other with a switch, showed differential preference (each preferring the new object) when faced with choosing one to function as a pendulum bob, whereas the choice frequencies of the control group were chance. The authors conclude that two types of learning are suggested by these results: the acquisition of "certain broad nonspecific, general notions" and the acquisition of experiences that change the "initial perception of broad general properties of an object into perception of specific limited functional characteristics." The former is necessary for productive thinking and the latter produces functional fixedness. Adamson & Taylor (2), also using the two-string problem, showed that functional fixedness decreases as the time interval between the initial use of the object and the test is increased. Five groups, tested after intervals of $\frac{1}{2}$ min., $\frac{1}{2}$ hr., 1 hr., 1 day, and 1 week ranked in functional fixedness in the predicted order except that the first two groups were tied. This finding is interpreted in terms of an increase in retroactive inhibition, resulting in increasingly greater interference with the induced fixedness.

These experiments leave no doubt as to the reality of the phenomenon described by Duncker. That functional fixedness is operationally similar to the inhibiting effect of set is strongly suggested not only by the fact that both decrease with elapsed time, but also by a second result reported by Adamson & Taylor (2). Their subjects were also given a Luchins series of problems. Significantly more of those lacking in the ability to overcome set, as measured by failure on the extinction problem, displayed functional fixedness on the two-string problem. However, susceptibility to set, as evidenced by performance on the critical problems, was unrelated to functional fixedness, a finding which re-emphasizes the importance of Guetzkow's (47) delineation of these two aspects of set and further justifies Luchins' (87) insistence that performance on the extinction problem rather than that on the criticals should be used as a measure of behavioral rigidity in problem solving. It is to be hoped that there will be no repetition of the Levitt & Zelen attempt (83) to show that Luchins' measure of "set" is not a valid measure of "rigidity."

OTHER STUDIES OF INDIVIDUAL PROBLEM SOLVING

Two papers deserve at least brief mention here even though both have been discussed in more detail in chapters on Learning in earlier volumes (59, 156). The first is Marks' study (100) of the effect upon achievement of various techniques designed to influence the subject's awareness of the elements of the problem. Particularly notable is Whitfield's experiment (162) which

employed problems whose difficulty could be measured theoretically at any successive stage in solution. The nature of the relation obtained between theoretical difficulty and actual performance shows the importance of memory and its limitations in problem solving. Stressing the relation of thinking and memory in a different sense, Cohen (24) has argued that emphasis has been placed too much on thinking as a function of the immediate situation and too little on thinking as a function of the individual's life history; analysis of the life history is particularly important, he believes, in understanding what makes people detect problems.

Critical thinking and logical reasoning.—Several studies have been concerned with critical thinking and logical reasoning. Performance on the Morgan Test of Logical Reasoning of a group which had had at least a semester of logic was found to be significantly superior to that of a matched control group [Morgan & Morgan (114)]; it was noted, however, that the performance of the control group was relatively good, its mean being 73 per cent of the mean for the other group. An increase of 62 per cent in performance on the Ohio State Psychological Examination by students taking a course in functional logic was interpreted by Melzer (108) as showing the efficacy of such logic in increasing critical thinking, although no control group was used. One may wonder whether the scales underlying these last two percentages have meaningful zero points. Brembeck (13) compared matched experimental and control groups in 11 colleges on a battery of four Watson-Glaser Tests of Critical Thinking administered before and after a semester course in argumentation. Significant differences favored the experimental groups in some cases and the controls in others. The correlations of A. C. E. Psychological Examination scores with various subject matter tests involving critical thinking were found by Furst (38) to be about the same as with those tests involving primarily knowledge or elementary skills.

Gorden (41) obtained some evidence that performance on syllogisms involving statements about Russia was affected by attitudes toward Russia. In a more adequate study, Thistlethwaite (148) employed a 72-item test involving six nonsyllogistic forms of inference. Data obtained from 559 college students in seven universities support the conclusion that groups from southern states differ significantly from northern groups in distortions of reasoning, both on an anti-Negro subscale and on a general ethnocentrism scale. An interesting method of analysis showed that as the structure of the form of inference became increasingly compatible with prejudiced responses, the degree of relative distortion increased.

The performance of 25 undergraduates on an induction, a deduction, and a unicursal figure problem was analyzed by Burack (15) in terms of degree of use of nine different "methods of attack." Both the extent of use and the potential efficacy of a particular method of attack varied with the kind of problem involved. Employing four tests previously shown by Guilford to have high loadings on logical reasoning and low ones on other factors, McNemar

(105) selected from 308 men and 180 women a high and a low group of 73 each. On a series of word-association tests, the two groups did not differ in speed of unrestricted word flow, but did differ in speed of controlled association, the difference increasing in significance with each increase in restriction upon association. To the two groups, McNemar also gave the induction and deduction problems used by Burack. The high group was superior both in accuracy and speed of induction, but only in accuracy of deduction. Differences between the two groups were found only for three of six methods of attack investigated, namely, control, locating a crucial aspect, and elimination of sources of error.

The effect of distribution of practice on solution of a deductive problem was investigated by Shaklee & Jones (139) using 300 subjects divided into three groups, all of which worked 20 min. The group which alternated 1 min. of work with 1/2 min. of rest was inferior to the other two groups, of which one worked continuously and the other alternated 1 min. of work with 1 1/2 min. of rest. Some question is raised by the fact that roughly 50 per cent of those questioned admitted practicing during rest intervals, but only about 10 per cent reported that the practice helped.

Thinking processes.—On the basis of a test of arithmetic problem solving, Moraes (113) selected from 840 Belgian school children 81 of the better and poorer students. Employing the "méthode de la réflexion parlée," she gave to these 81 individuals six arithmetic reasoning problems of difficulty appropriate to their grade in school. The protocols obtained for each of the three levels of problem difficulty are presented at length to show the nature of thinking processes involved.

By an ingenious variation and analysis of the familiar coin weighing problem, Simmel (141) was able to determine experimentally the influence of three tendencies in the thinking of subjects solving the problem. Evidence was found for both the tendency to balance equal number of coins (symmetry) and the tendency to use all the coins (totality), but not for the divisibility tendency. She argues that these tendencies are not the result of needs or of habit or set in the usual sense, but that these psychological properties of the problem depend upon past experience in the wider sense.

In an investigation using college students Bloom & Broder (12) also used the method of "thinking aloud." They report a qualitative analysis contrasting the processes of six academically successful with six unsuccessful students and another showing how the thinking processes of a group of 27 entering students changed over a period of six to eight weeks. They suggest that "thinking aloud" should be used in constructing test items to gain insight into the kind of problem a particular item actually represents to students.

Teaching problem solving.—To 10 students whose achievement level was below their aptitude level, Bloom & Broder (12) gave six weeks of individual remedial training in problem solving. This experimental group showed significant improvement in grades, whereas control groups matched for scholastic

aptitude or for initial grades did not improve. Less satisfactory results were obtained in a second study employing group remedial training. Calvert *et al.* (20) found that college freshmen, taught drawing by techniques designed to improve problem solving skills in engineering, were superior to a similar group taught by the usual method, in a test of flexibility of approach to drawing problems. But the two groups did not differ either on a test designed to measure conventional coverage or on a special test of problem solving ability.

Katona's card trick experiments have been repeated and extended by Hilgard *et al.* In one experiment (70) a group of 30 high school students individually learned the first two card tricks by rote and another group of 30 were taught by a method designed to produce understanding. More time was required to teach the problems to this latter group. The two groups did not differ significantly in retention overnight, but the understanding group showed greater transfer to three new card trick problems. In a second experiment (71) significant differences in transfer of training among five different methods of teaching understanding of the card tricks were found for only one of four transfer problems. Analysis of errors made supported the impression that casual circumstances of method and attitudes of caution or carelessness were more important in determining level of error than was lack of understanding.

Brief mention may be made of two papers by Polya, the well-known mathematician and author of the book, *How to Solve It* (121). The first paper (122) presents the same proof of an elementary theorem in two ways, "first briefly without motivation, then broadly with motivation," and stresses the importance of presenting proofs in a manner such that the student sees the relation of each step to the final goal. The second paper (123), which in part emphasizes the importance of teaching students to distinguish more reasonable from less reasonable guesses, ends with, "Let us teach guessing!"

Trouble shooting.—The diagnosis of difficulty in malfunctioning equipment is a type of problem solving of considerable importance in military situations and has been the subject of a number of recent studies. Harlow (61) has presented a series of diagrams developed by Gagne and his associates in the analysis of the sequential processes involved in trouble shooting and also in interpretation of aerial photographs. The diagrams are based on the communication theory model in which the checking or verifying procedures are assumed to be binary operations which eliminate each time half of the remaining hypotheses. Parenthetically, it may be noted that Taylor (145) has called attention to relevance of such a model to the game of Twenty Questions. Bendig (10) has employed the game modified in such a way that the data obtained would be amenable to information analysis: the subjects guessed animal topics from a list of 16 after each of four questions asked and answered by the experimenter. The results indicated that the subjects used about 80 to 90 per cent of the information provided by three questions but only about 20 per cent provided in a fourth question.

In an experiment with 54 undergraduate students, Fattu & Mech (34) demonstrated that a tape recorded basic knowledge lecture explaining the nomenclature and functioning of a gear-train apparatus produced no improvement in trouble shooting ability. However, a basic knowledge lecture plus a symptom-analysis lecture did produce significant improvement.

In a thorough and careful study, Miller, Folley & Smith (111) have presented evidence to support the view that trouble shooting by "logical elimination of possibilities of malfunction sources" is a more workable and effective procedure for maintenance of electronic equipment than is the method of "trouble shooting from probability data." The latter method involves a checking procedure based on the probability of association of causes of malfunction with given symptoms. A mathematical proof is presented showing that, given certain assumptions which are not always met, a so-called half-split technique closely related to information theory is an efficient procedure. In theory the technique permits handling any distribution of probabilities of malfunction of individual units, but in practice equal malfunction probability of each component will ordinarily be assumed.

Space does not permit an adequate description of two newly developed instruments, both of which appear particularly promising for use not only in studies of trouble shooting but also in other investigations of problem solving. One of these is the Tab Item technique described by Glaser, Damrin & Gardner (39); the other is the Multiple-Alternative Symbolic Trouble Shooting Test (MASTS) developed by Grings and his associates (46).

Language and thinking.—A report has been published of only three of a larger number of experiments on language and thinking carried out by Cofer and his associates. Because the eight technical reports describing these experiments are out of print and because space is not available here for an adequate summary of all of them, only those summarized by Cofer (23) will be cited. The scarcity of work in this area makes publication of a full report of these experiments particularly desirable. Only one related paper (25) within the period under review has come to the attention of the authors.

In one experiment, three groups of subjects were asked during five successive class hours to learn eight lists of words and during a sixth period to solve the Maier two-string problem. For one group, words such as rope, swing, and pendulum occurred together in one list, whereas for the other two groups, rope was associated with hemp, for example, or pendulum with clock. The first group gave more pendulum solutions to the two-string problem, thus confirming the hypothesis that strengthening verbal associations will affect problem solution. In a second experiment, groups of subjects were presented with 48 items of the form: skyscraper, temple, cathedral, prayer. The task was to select the word which should be excluded. Note that the exclusion may be on the basis of either the "religion" or the "building" category. The hypothesis was that the word chosen as "not belonging" would depend on the set produced by the order of the two unambiguous words, skyscraper

or prayer, in the item. This hypothesis was confirmed by data obtained for 12 groups of 25 subjects; forms of the test were employed rotating the two unambiguous words through every possible position. In the third experiment, subjects were first trained to choose one of four words as "correct." On succeeding items involving single trials without reinforcement, they tended to choose the one of the four words which was associated with the "correct" word on the first trial, as shown by the results of a free association test given earlier.

In his book, *Language and Communication*, Miller (110) has made the stimulating suggestion that the concept of "recoding" can often be substituted for the concept of "restructuring" as used in field theories of thinking. Thus, for example, the number, 14,916,253,649, may be difficult to remember unless one "restructures" or "recodes" it as simply the square of each of the digits in order from 1 through 7. He points out that "the recoding principle is one of the most powerful tools of science." Its implications for the psychology of thinking are particularly interesting.

Sex differences.—In three experiments previously cited and primarily concerned with other variables, sex differences were reported. On the test of arithmetic problem solving administered by Moraes (113) to 840 Belgian school children, the achievement of boys was slightly superior, but significantly so for only one of the three age levels tested. Her data lead her to suggest that girls reason as well as boys, but that they are either less quick or more careful; this suggestion is supported by Sweeney's finding (144) in one experiment that larger sex differences are obtained with shorter time limits. On six card trick problems involving retention and transfer, Hilgard *et al.* (70) found the performance of 75 high school boys to be superior ($P = .001$) to that of 75 girls drawn at random from the same school. In Cofer's experiment (23) employing the Maier two-string problem, women gave relatively few pendulum solutions, and the differences among the experimental groups described above were not significant for women but were for men. Saugstad (136) obtained a correlation between incidental memory and problem solving of $-.41$ ($P = .01$) for 49 high school boys, but of only $-.10$ and not significant for 21 girls.

Sweeney (144) has reported a series of experiments designed to determine whether the kinds of sex differences obtained incidentally in earlier experiments could be verified and whether such differences could be demonstrated for groups of men and women matched with respect to general intelligence, various special abilities, or specific knowledge or training. Sweeney was able to obtain scores on the College Board Scholastic Aptitude Test for 130 men and 139 women to whom McNemar (105) had given four tests of logical reasoning: False Premises, Essential Operations, Syllogisms, and Problem Solving. Significant differences favoring the men were found on all four of these tests for 100 pairs matched in verbal aptitude scores. For 90 pairs matched in mathematical aptitude and for 69 pairs matched both in verbal and in

mathematical aptitude, a difference was obtained only for Problem Solving ($P < .0002$), a test which involves essentially arithmetic reasoning. In two experiments employing the classic Alcuin problem, Sweeney used subjects drawn at random from a population in which there was known to be no sex difference in intelligence. In both experiments, the subjects were first equated in specific knowledge by training them to solve a simple version of the problem and then were tested on a more complex form, differing only in that it required an additional detour step. In both, the men achieved a larger percentage of solutions ($P = .005$) on the more complex form.

In Sweeney's most elaborate experiment, large samples of men and women were given verbal tool problems, puzzle-book problems, and mathematic-logic problems, as well as the second part of Terman's Concept Mastery Test, Thurstone's Flag Test, an Abbreviated Mechanical Comprehension Test, and a Mathematics Achievement Test, and information was obtained concerning their training in mathematics. Significant sex differences were obtained for groups matched in general intelligence, spatial ability, mechanical comprehension, mathematics achievement, or amount of training in mathematics. In general the results confirmed the hypothesis that sex differences will occur in problems which involve difficulties in restructuring, but not in similar problems which involve no such difficulties.

GROUP PROBLEM SOLVING

In recent years an increasing number of experiments have been concerned with the behavior of small groups. Those published before 1952 have been reviewed by Roseborough (134). Of such experiments only a fraction, of course, have concerned problem solving [see forthcoming review by Kelley & Thibaut (78)]. Even many of the studies of groups solving problems or making decisions have been designed to study leadership, motivation, or the effect of personality or cultural variables, and as a result they have employed no measure of achievement in terms of problems solved, quality of decisions made, etc. Attention will be limited here mainly to those studies of group problem solving employing some measure of achievement and concerned primarily with variables which affect such achievement.

Restriction of communication.—Four different communication patterns were investigated by Leavitt (81) in a study using 20 groups of five male undergraduates each. Each subject in a group was given a card on which appeared five, out of a possible six, symbols. The group problem was to find the one symbol common to the cards of all five members. Communication was permitted only by written messages along specified channels. The circle pattern resulted in the largest number of errors and the Y and wheel patterns in the fewest. The different patterns did not differ significantly, however, in the time required to finish the problem. In terms of a variety of measures of behavior, the four patterns tend to fall in the order, circle, chain, Y, wheel. "The circle, one extreme, is active, leaderless, unorganized, erratic,

and yet is enjoyed by its members. The wheel, at the other extreme, is less active, has a distinct leader, is well and stably organized, is less erratic, and yet is unsatisfying to most of its members" (81, p. 46).

Heise & Miller (69) measured the performance of three-man groups for five different communication nets, three signal-to-noise ratios, and three kinds of tasks. The first two types of problems required communication among members of the group for solution: one type involved simple reassembling of a list of standard words, while a second involved construction of a sentence from words which had been distributed among the group members. The third type of problem, anagrams, did not require communication. For the first type of problem, a closed chain where only one-way communication was possible between any two members was by far the least efficient, and a closed chain where all members talked and listened to all other members was most efficient. In contrast, most efficient for the second type of problem was the net in which the man in a central coordinating position had two-way communication with the other two men. The second type of problem was less structured and hence perhaps more dependent for solution upon the coordination of the group activity. As was expected, the anagram problem which did not require communication resulted in no large differences among the high communication nets. Increasing stress by reducing the signal-to-noise ratio accentuated the differences among the five nets for the first two types of problems. Heise & Miller conclude that "the performance of a small group depends upon the channels of communication open to its members, the task the group must handle, and the stress under which they work" (69, p. 335).

Bavelas (8) used a problem in which five squares of cardboard were cut up and the pieces distributed among the members of five-man groups. The task was to redistribute the pieces over open communication channels until all five members could form a square. Individual members of the group could form squares in ways which made it impossible for the remaining members of the group to do so. The member who formed a square in a way which blocked the group's solution was understandably hesitant to break it up and would do so only if he had adequate knowledge of the total situation. Any severe restriction of the communication channels was found to make solution of the problem by the group almost impossible.

Methods of leading discussion.—An experiment by Maier (93) was concerned with the influence of the discussion leader upon the quality of group decisions. The problem for discussion involved a seven-man assembly line which was being slowed down by "Joe," an older man who was not able to keep up with the rest of the group. College students attempted to solve the problem under one of four experimental conditions. Under the first two, they worked alone, in one case without and in the other with knowledge of the roles of the seven men on the production team. Under the latter two, they worked in groups of seven, in one case with untrained and in the other with

trained leaders; in all groups, each of the seven members acted out one of the roles of the production team. None of the 31 individuals working without knowledge and only one of the 42 individuals working with knowledge succeeded in obtaining what was judged to be the best solution to the problem. Only one of the 29 groups led by untrained leaders obtained the best solution, whereas 7 of the 11 led by trained leaders obtained the best solution. The interpretation of this finding, however, is limited by the fact that the well-trained leaders knew the best solution in advance. Moreover, only 76 per cent of the groups with untrained leaders as compared to 57 per cent of the individuals with knowledge of roles obtained either the best or the second best solution to the problem, a difference which computation shows is not significant at the .05 level. Somewhat contrary to Maier's emphasis, the most defensible interpretation of the data would seem to be that more correct solutions were achieved by groups led by trained leaders with knowledge of the correct solution than by groups with untrained leaders lacking knowledge of the solution or by individuals working alone.

In a study of Maier & Solem (95), each of 67 groups containing five or six students first elected a representative. The experimenter then instructed the representative in 33 of the groups to function only as an observer and to refrain from expressing any views of his own; for the other 34 groups, the representative was instructed to function as a discussion leader, avoiding expression of his own views, but providing a permissive atmosphere for the expression of the views of others. An eight-minute discussion period for the groups with leaders was found to produce a significantly larger increase in number of solutions by individual members of an arithmetic reasoning problem. In most instances a minority with the correct answer was more influential in the groups with leaders than in the groups with observers, indicating that "a discussion leader can function to upgrade a group's thinking by permitting an individual with a minority opinion time for discussion" (95, p. 287).

In an interesting experiment by Torrance (151), 57 combat aircrews (10 to 15 men) were divided randomly into four experimental groups and one control group. Both experimental and control groups were given two tests, each of which presented problem situations too complex for solution by any step-by-step logical reasoning process and required the selection of the most essential of the many elements presented in the problem situation. Following the administration of the first problem solving test, the performance of each experimental group was criticized by one of four methods. In two of the methods, the experimenter used nonauthoritarian or crew-centered techniques to get the crew to evaluate its own performance. One was unstructured, whereas the other was structured in that a set of rating scales was used as a guide by the crew in evaluating itself. A directive method was employed in which the experimenter diagnosed the performance of the crew using the same rating scales, pointed out ineffective procedures, and suggested ways of improve-

ment. Under the fourth experimental condition, the experimenter simply left while the group conducted its own self-critique. After each of the two problem-solving tests, experimenters rated the groups on five-point scales for each of 13 characteristics of performance (e.g., organization of manpower, selective use of personnel, and so forth). Crews criticized either by the directive or by the structured nonauthoritarian method were found to show significantly greater improvement in performance than crews criticized by the other two less structured methods. These latter showed no greater improvement than the control groups. This is true both for performance scores based on a combination of the 13 ratings and also for scores representing achievement in solving the problems.

McCurdy & Lambert (104) appointed leaders for each of 12 groups of three, the other two members of each group being directed to follow the directions of the leader. The achievement of these 12 groups in solving one type of problem did not differ from that of 10 groups in which no leader was designated.

An intensive field study of 72 actual conferences held in business and government was carried out at the University of Michigan. The conferences selected for study had as their primary purpose the making of decisions and involved from 5 to 17 participants who had worked together in previous meetings. Since only a partial report of the findings is thus far available [Marquis, Guetzkow & Heyns (102)], the study will not be summarized here. The final report should be of major interest.

Group processes.—Bales & Strodtbeck (4) have advanced the hypothesis that under specified conditions the process in group problem solving will tend to move from a relative emphasis upon problems of orientation, to problems of evaluation, and then to problems of control and that concurrently the relative frequencies of both negative and positive reactions will increase. The behavior of 22 groups had earlier been recorded from firsthand observation in terms of 12 different categories that were in turn grouped into five classes more general in nature. Of the 22 groups, only 8 met the specified conditions, which are too elaborate to summarize here. For 6 of these 8, the hypothesis was confirmed at or beyond the .05 level. For none of the 14 which failed to meet the specified conditions was the hypothesis confirmed. One question should be raised. The statistical test used involves essentially a repeated application of Kendall's rank correlation coefficient tau. If this procedure involves the questionable assumption that the frequency of occurrence of behaviors in one of the five general classes (e.g., orientation) is independent of the frequency in each of the other four classes, the significance of the results for the eight groups meeting the conditions may be overestimated.

Group versus individual problem solving.—Taylor & Faust (147), employing problems of the type involved in the game of Twenty Questions, gave four problems a day for four days to 15 individuals, 15 groups of two, and 15 groups of four. Group performances were found to be superior to individual

performance in terms of number of questions required for solution, number of failures, and elapsed time per problem; but the groups of four were not superior to groups of two except in solutions reached. In terms of man minutes required for solutions, the performance of individuals was superior to that of groups of two and the performance of groups of two in turn superior to that of groups of four. McCurdy & Lambert (104) compared the efficiency of groups of three with that of individuals in the solution of a type of problem which they believed to require genuine co-operation. The problem required each member of the group to manipulate two switches in a manner such that the group's six switches would match a predetermined pattern of manipulation of the experimenter's six switches. In two experiments, one involving 11 individuals and 13 groups and the other 12 individuals and 22 groups, the achievement of individuals was superior to that of groups although the difference did not reach significance in the latter.

As many have previously pointed out, the performance of a group should be superior to that of an individual on many kinds of problems simply because in the group more individuals are working on the problem. Taylor (146) has recently presented a simple mathematical model for predicting the performance of a group of given size from a knowledge of individual performance on the assumption of the appropriate null hypothesis, namely, that working in a group has no effect either positive or negative upon individual performance. Assume that of a given population of subjects, p will solve and q will fail a particular problem. The probability of drawing one subject from that population who will fail the problem then is q . The probability of drawing two subjects, both of whom will fail the problem, is q^2 ; of drawing k subjects, all of whom will fail the problem, is q^k . Assume also, as is logical under the null hypothesis, that if any individual in the group solves the problem, the group has solved it. The probability then that a group of k size will solve the given problem will be equal to $1 - q^k$. Using this model, one may by sampling determine the proportion of individuals who will fail a problem, the proportion of groups of any given size which may be expected to solve the problem, and compare this predicted proportion with the proportion of groups of that size which actually solve the problem. Significant deviation of the obtained from the predicted proportion is evidence of group interaction.

Unfortunately, the statistical procedures developed (84, 146) for testing this deviation do not permit combining the results from several problems for the same individuals or groups. Accordingly, because of the unreliability of performance on a single problem, a very large number of subjects working either on a single problem or divided into smaller groups each working on a separate problem is necessary to permit an adequate test of the null hypothesis using this procedure. For this reason Taylor (146) has described an alternative method for testing the same hypothesis. Individuals are randomly assigned to work either alone or in groups of a given size on a series of prob-

lems. The number of individuals working alone should be about equal to that working in groups. After the experiment is completed, those who actually worked alone are divided at random into "nominal" groups of the same size as the real groups. The performance of these nominal groups is scored by assuming that if any one in the group solved a particular problem, the group solved it. The t test may be applied to test the difference between the mean number of problems solved by the real and the mean number solved by the nominal groups.

Lorge & Solomon (84) independently formulated the same model [$p_g = 1 - (1 - p_i)^k$] described by Taylor (146) and employed it in a reanalysis of the data from the classic Shaw experiment (140). For one-step problems the second model reduces to the simpler one just described. The second provides a prediction of the behavior of the group on the assumption that only one kind of group interaction is occurring, namely, the pooling of the abilities of the members of the group. Suppose, for example, that a particular problem is solved in two steps. It may be expected that some subjects will be able to solve both steps, some neither, and some only one or the other. If a particular group contains at least one member who can solve the first step and at least one member who can solve the second step, then the group will solve the problem even though no member of the group can solve the problem as a whole. This pooling of ability is a type of group interaction which may account for the superiority of groups in situations in which their achievement is significantly better than that which would be predicted from the simpler model. In using the more elaborate model, Lorge & Solomon (84) were able to make an acceptable prediction of group achievement for the one of the three Shaw problems for which the simpler model had previously been rejected. For the two problems for which the simple model had provided an acceptable prediction, the more elaborate model yielded predictions which more closely fit the values actually obtained.

Two limitations of the use of the ability-interaction model deserve mention. First, even though this model may provide an acceptable prediction of group achievement on problems for which the simpler model is rejected, this does not prove that the superior group performance was in fact the result of ability interaction. Models based on the assumption of other types of interaction ("dynamic"?) may provide an equally acceptable prediction of the same superior performance. However, the burden of proof would seem to be on those who assume such interaction. The second limitation is a practical one. In using the model, one must obtain estimates of the proportions of individuals in the population who can solve each combination of steps in the problem, e.g., the first and not the second, the second and not the first, and neither or both. To determine these proportions empirically would be laborious for the two-step problem. For a problem involving more than three steps, the amount of work appears prohibitive.

Using the design involving nominal groups described above, Faust (35)

* They have also developed a second and more elaborate model. See Errata before Contents v. 7, 1956.

found that for three spatial problems the performance of groups of four was not significantly better than that to be expected on the basis of the null hypothesis represented by the simpler model, but that for verbal problems the null hypothesis had to be rejected. It is interesting to note that the three spatial problems which he employed were of the type which appear to be solved in one step, whereas the four verbal anagram problems were clearly solved in several steps. It seems quite plausible that the significant positive group effect obtained with the verbal problems was the result of pooling of ability and could be accounted for in terms of the ability-interaction model of Lorge & Solomon. Even for problems in which there are positive effects of working in a group, these effects may often be accounted for in terms of statistical rather than psychological interaction.

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